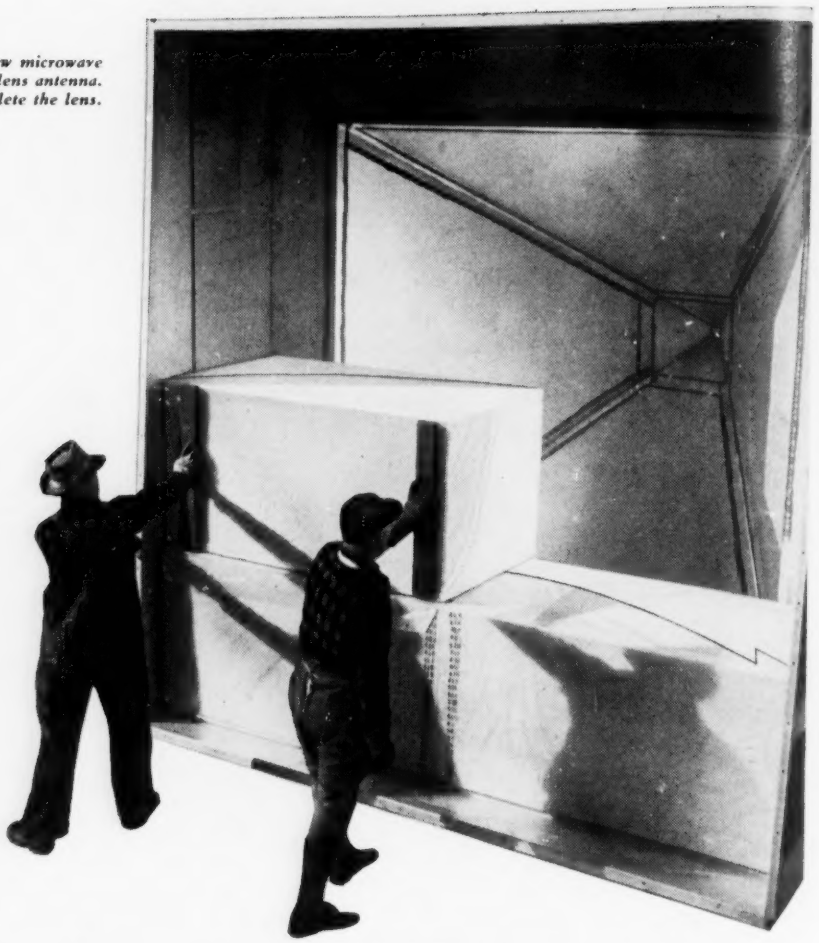
VOL. LXXI, NO. 3 SEPTEMBER 1950

*Mounting Bell's new microwave lens in a horn-lens antenna. Other blocks will complete the lens.*



# A focus on better, low-cost telephone service

In the new microwave radio relay system between New York and Chicago, giant lenses shape and aim the wave energy as a searchlight aims a light beam.

Reasoning from the action of molecules in a glass lens which focuses light waves, Bell Laboratories scientists focus a broad band of microwaves by means of an array of metal strips. To support the strips these scientists embedded them in foam plastic which is rigid, light in weight, and virtually transparent to microwaves.

This unique lens receives waves from a wave guide at the back of the horn. As they pass across the strips, the waves are bent inward, or focused, to form a beam like a spotlight. A similar antenna at the next relay station receives the waves and directs them into a wave guide for transmission to amplifiers.

This new lens will help to carry still more television and telephone service over longer distances by microwaves. It's another example of the Bell Telephone Laboratories research which makes your telephone service grow bigger in value while the cost stays low.

*Laboratory model of the new lens. A similar arrangement of metal strips is concealed in the foam plastic blocks in the large picture.*

BELL TELEPHONE LABORATORIES

*Working continually to keep your telephone service big in value and low in cost.*

# THE SCIENTIFIC MONTHLY

VOL. LXXI SEPTEMBER 1950 NO. 3

## *Historical Issue*

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***Annual Meeting, AAAS, Cleveland, Ohio, December 26–30, 1950***

GLADYS M. KEENER, Executive Editor, THE SCIENTIFIC MONTHLY

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# Science and Technology in 1873

(*From* The Popular Science Monthly, *1873, Volume III*)

### Marked Case of Heredity in Mastiffs

Mr. Darwin communicates to *Nature* a letter from Mr. Huggins on the hereditary transmission, in a breed of mastiffs, of a strong antipathy to butchers and butchers' shops. Mr. Huggins owns a dog, "Kepler," whose sire was a celebrated mastiff, "Turk." When "Kepler" was six months old he followed a servant out on the street, and then for the first time saw a butcher's shop. The animal threw himself down, and could not be induced to pass the place. The dog is now nearly three years old, and the antipathy has diminished somewhat, but not disappeared. Mr. Huggins lately found that "Kepler's" ancestor, "Turk," manifested the same antipathy, and his former owner was asked for information on the subject. It now appears that this curious dislike for butchers' shops and butchers was shown equally by "Turk's" sire, "King" (in whom it probably originated), and by "Punch" and "Paris," sons of "Turk." The antipathy is most marked in "Paris," who will hardly enter a street containing a butcher's shop, and runs away after he has passed it. If a butcher's cart comes to the place where the dogs are kept, they are filled with fright even though they do not see the object of their fears. "Turk's" owner, Mr. Nichols, then tells of two instances where "Paris" gave evidence of the most extraordinary sagacity in recognizing a butcher under any circumstances. One evening a boss-butcher, in ordinary clothes, called to see "Paris," but had scarcely entered the house when the dog became unmanageable, and the visitor had to leave without seeing him. On another occasion "Paris" sprang at a gentleman, and, as it was the first exhibition he ever had made of such viciousness, his owner apologized, and said that the dog had never before attacked any but butchers. The gentleman *was* a butcher!

Since the publication of Mr. Huggins's letter, several other communications have appeared in *Nature,* showing that all the dogs of this line inherit this instinctive antipathy. Mr. H. G. Brooke writes of a grandson of "Turk": "Even since he was a pup he has evinced" this antipathy. A brother of this dog of Mr. Brooke's shows the same feeling, according to Mr. Arthur Ransom, his owner.

Mr. Russel Wallace is inclined to think that these dogs distinguish butchers from other men by the sense of smell, which is very acute in all dogs. He also thinks that it is this sense which enables a dog to find his way back from a distance, though on first making the journey he had been blindfolded, and so prevented from seeing his way. Another correspondent of *Nature,* writing in confirmation of Mr. Wallace's view, tells of a cat's antipathy to dogs. This animal would "swear," if only stroked by a hand which had directly before touched a dog. Mr. Darwin's purpose in calling attention to the present case of heredity is to illustrate his theory of instinct as an acquired and transmitted habit.—p. 126.

### Debauchees

One of the great dangers attending the use of the various sedatives employed in the nursery is that they tend to produce the *opium-habit.* These quack medicines owe their soothing and quieting effects to the action of opium, and the infant is by them given a morbid appetite for narcotic stimulants. The offering for sale of such nostrums should be prohibited, as tending to the physical and moral deterioration of the race. In India mothers give to their infants sugar-pills containing opium, and the result is a languid, sensual race of hopeless debauchees. In the United States the poisonous dose is administered under another name; but the consequences will probably be the same.—p. 784.

### Machine-made Barrels

Patents have been issued in France for a series of machines capable of manufacturing all kinds of casks and barrels. The inventors, Messrs. Thuillier and Gerard, of Boulogne, recently gave a public exhibition of their machines, and turned out in a comparatively short space of time a large number of barrels. The machine-made ware is said to be better than that made by hand.—p. 399.

### Snakes Swallowing Their Young

The question, "Do snakes swallow their young?" that is, give them shelter in the maternal stomach when danger threatens, was discussed in a paper presented to the American Association by G. Brown Goode. The author some time since asked, through the public press, for testimony bearing on this subject, and he now comes forward with what appears to be perfectly satisfactory evidence in favor of the affirmative side. He has the testimony of fifty-six witnesses who saw the young enter the parent's mouth. Of these fifty-six, nineteen testify that they heard the parent snake warning her young of danger by a loud whistle. Two of the witnesses waited to see the young emerge again from their refuge, after the danger was past; and one of them went again and again to the snake's haunt, observing the same act on several successive days. Four saw the young rush out when the parent was struck; eighteen saw the young shaken out by dogs, or escaping from the mouth of their dead parent. These testimonies are confirmed by the observations of scientific men, such as Prof. Smith, of Yale College, Dr. Palmer, of the Smithsonian Institution, and others.—p. 783.

### The Failure of Car-Axles

The fracture of car-axles, and the frequent accidents arising therefrom, are due, it appears, in the majority of cases, to imperfect construction, which may be readily detected by applying the proper tests. As an example of the kind of work that manufacturers sometimes turn out to railway companies, we are told by Mr. James E. Whitney, in the *Railway Times,* that, of a lot of axles furnished to the Mobile & Ohio Railroad Company, but one-fourth were capable of meeting the required tests, . . . Mr. Whitney also says that the duty of making these tests belongs to the railway companies themselves, which leaves them no valid excuse for the employment of defective materials . .

But, however strong originally, car-axles always deteriorate with use, the constant succession of jars to which they are subject gradually impairing the strength of the iron. The character of this change is not well understood, and the only effective method now known of guarding against the danger arising from it, is to throw the axle aside after it has been run a certain number of miles.—p. 125.

# FOREWORD

THE mid-century year is an appropriate time for retrospection and some introspection. It was, I am sure, a desire simply to review the progress of science as reported in the pages of THE SCIENTIFIC MONTHLY and its predecessor, *The Popular Science Monthly,* that prompted the editors to plan the current number. Some of the results were not foreseen as an outcome of the project, but they possess sufficient interest and significance to warrant the space they have been given in the ensuing pages.

The initial purpose was to reprint a few of the articles that have stood the rigorous test of time. The only self-imposed limits were the contents of the two journals and the year 1872, when Edward Livingston Youmans founded *The Popular Science Monthly* as a D. Appleton & Company publication. Born in 1821 in the geological type locality of Coeymans, near Albany, New York, E. L. Youmans was denied his wish to become a practicing scientist by persistent ill-health and poor eyesight, which recurrently approached blindness. With the aid of a devoted sister and, later, of a sympathetic wife, he followed the progress of science to a degree that enabled him to publish a *Chemical Chart,* a *Class-Book of Chemistry,* a *Chemical Atlas,* and even a *Handbook of Household Science.* Conversant with the science and the scientists of his time, Youmans wanted to give the educated public authoritative information about the newest developments in scientific fields, and *The Popular Science Monthly* became the vehicle to achieve this end. Within five years the regular issues of 128 pages (or more) seemed insufficient for this purpose, and 96-page monthly *Supplements* were issued at twenty-five cents each, to acquaint the American public with the work of foreign scholars; for, as the editor wrote by way of introduction, "science is of no nationality."

There were few American media for scientific articles in 1872, and Youmans found it relatively easy to persuade top-rank American, British, and French scientists to write for the magazine. Or, failing to secure an especially written article, he did not hesitate to reprint important contributions from journals and reports not readily accessible to his discriminating audience. Significant books were reviewed; discussion and debate were welcomed; science was found then to be "on the march" and was so reported. Although flying saucers had not yet been observed, other, equally strange and wonderful phenomena were solemnly recorded, and gadgetry—though not mass-produced—was no less rampant.

Here, then, in the pages of *The Popular Science Monthly,* were all the ingredients of its lineal descendant, THE SCIENTIFIC MONTHLY. The tradition was preserved by J. McKeen Cattell, not only when he took it over in 1900, but also in 1915, when he abandoned the old name and rechristened the journal. This month's issue of THE SCIENTIFIC MONTHLY is composed wholly of articles and items from the early volumes of the latter and its distinguished progenitor. The illustrations in the article by Powell are the original ones.

Instead of offering this Historical Issue complacently to our modern readers, as evidence that their journal has pioneered in the presentation of important facts and brilliant theories, from Volume I of *The Popular Science Monthly* to Volume LXXI of THE SCIENTIFIC MONTHLY, the editors submit it humbly, in the full awareness that there's nothing much new under the sun. Indeed, some of the arts of the nineteenth century appear to have been lost. The art of good writing may be one of them. Only one typographical error was turned up in a great deal of reading. And E. L. Youmans issued a 96-page *Supplement* for only twenty-five cents.

Scientifically, old quarrels like the one over vivisection still rage, and others have merely shifted ground. Many modern views sound suspiciously like old ones disguised in specialized terminology. If the reader is tempted to laugh at certain of the naïve arguments and conclusions, he will, at the same time, feel rather uncomfortable about some of his own convictions. He will certainly be impressed with the fact that academic freedom was as troublesome a problem in the seventies as in the memorable days of the Scopes trial and later—and for the same reasons.

Although the issue contains items that span half a century, it is believed that each of today's readers will still feel he is reading THE SCIENTIFIC MONTHLY. As for the editors, it is their hope that they will be inspired to make subsequent issues all the better for having delved into an impressive past.

HOWARD A. MEYERHOFF

*Chairman, AAAS Editorial Board*

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THE
SCIENTIFIC MONTHLY

SEPTEMBER 1950

# Vivisection*

MICHAEL FOSTER, M.D., F.R.S.
*Professor of Physiology in the University of Cambridge*

IN THE following pages I propose to inquire whether it is desirable that physiologists should continue the practice of what is commonly called vivisection, to which they have hitherto been accustomed. By vivisection I understand the operating with cutting instruments or by other means on the still-living bodies of animals. The word "living" requires, perhaps, some further definition. In the long series of changes through which the body of a living animal passes, from full functional activity to complete decomposition, there are three chief stages, each of which may be arbitrarily taken as the end of life. There is the time at which consciousness is lost, the time at which the breath stops and the heart ceases to beat, and the time at which the muscles become rigid with the death-stiffening. The succession of the three events is always in the same order, but the interval of time between any two of them varies within very wide limits. For our purposes it will perhaps be best to take the second as marking the end of life, to say that an animal is still alive so long as the heart is beating, and air enters into, and issues from, the chest.

It is very desirable that a discussion, the decision upon which must be of the utmost importance to physiology at least, should not be turned aside to any false issues. The question whether vivisection is a bad thing is in no wise settled by asserting that there are many things equally bad. Thus, to say that the evil wrought upon animals in the name of science is but a fleabite compared to that done in the name of sport, is simply to bring forward a *tu quoque* argument of no real worth, except to stop the mouths of particular opponents. When an ardent sportsman, or when one, no sportsman himself, but having a theoretical admiration of the pleasures of the field, declaims against vivisection, it may be worth while to remind such a one of some of the agonies of sport—of the scenes which accompany a battue or a pigeon-match; of wounded birds dragging their maimed bodies to some hidden covert, there to die a lingering death; of the piercing squeals of the hunted hare; of the last moments of the brave fox, when, after a fruitless struggle, the time comes for his living body to be torn by the pursuing hounds; to ask him how often a living object of sport is by some purposeful, sudden blow, humanely killed "to put it out of its misery;" to suggest to him, as a matter of reflection, that, had we any satisfactory measure of pain, it would be

◀ Officers of the American Association for the Advancement of Science at Ann Arbor Meeting in 1885. *Seated, left to right*: F. W. Putnam, of Cambridge, Permanent Secretary; Chas. S. Minot, of Boston, General Secretary; T. Sterry Hunt, of Montreal, a Past President; H. A. Newton, of New Haven, President; Jas. Hall, of Albany, a Past President; W. H. Walmsley, Secretary, Section G; Mrs. Erminnie A. Smith, Secretary, Section H; and C. J. H. Woodbury, Secretary, Section D.
*Standing, left to right*: Edw. Atkinson, Vice President, Section I; F. P. Dunnington, Secretary, Section C; N. T. Lupton, Vice President, Section C; Wm. Harkness, Vice President, Section A; E. W. Hyde, Secretary, Section A; J. Burkitt Webb, Vice President, Section D; C. W. Smiley, Secretary, Section I; S. H. Gage, Vice President, Section G; J. Owen Dorsey, Vice President, Section H; T. J. Burill, Vice President, Section F; and J. A. Lintner, Secretary, Section F.

* Reprinted from *The Popular Science Monthly*, 1873–4, 4, 672–85. Selected by Gladys M. Keener.

found that all the pain which physiologists have caused, since their science began, is less than that which the animal creation has suffered in the field from the hands of the members of the two Houses of Parliament since the last general election. It may be of use to say this to a sportsman; but vivisection is not thereby justified. It is no use saying it at all to those who are now agitating this question. They are as equally opposed to cruelty in sport as to cruelty in science; but they are also wise in their generation. They see that there is far more hope of putting down the one than the other. Biologists and physiologists are at the present moment clearly in disrepute. To call them atheists is to show onself a man of spirit and intelligence. Following out their own science, along the path Nature has pointed out to them, they have run counter to many established opinions and cherished views. Divorced by the divergence of their respective methods in large measure from the mathematicians and physicists, to whom orthodoxy is easy, accused of materialism, active in the support of Darwinism and evolution theories, believed by the many to have no faith—their position not a little resembles that of the Jews in the Middle Ages; they are just in the condition in which the accusation of cruelty is most tellingly made and most readily credited against them by a vulgar public. This the opponents of vivisection know full well; and therefore it is against the physiologists and not against the pigeon-shooters that they make their complaint. They are even willing, at the present, to use the latter against the former. By and by, if they are successful in this, they will move against sport, on the ground that it is far more cruel and has far less justification than the vivisection which has been done away with.

Nor is it any use to tell a far larger class, the eaters of meat, that the pain which physiology has caused since the time of Galen is far less than that which in any one week is caused in butchers' shambles in providing flesh to fill the mouths of the people of London.

Nor is it, on the other hand, any use to say that because many physiologists are kindly, humane men in private life, therefore the accusation of cruelty brought against them must be false. I know a physiologist who, after a day spent in experimental work, may be seen sitting in the evening with a favorite cat on his lap, an old dog by his side, and a new one at his feet; but I would not therefore guarantee that he had not been cruel in the morning. He might be an angel in the bosom of his family, but a demon in the laboratory. I know a physiologist of whom his friends have said that, had he not been so amiable, he might have made a noise in the world, and yet who at the present moment is being accused of brutal cruelties. I feel the accusation might be true.

Nor is it of any use to say, though it may be said with perfect truth, that a great deal of the present agitation against vivisection is one of the many fruits of a mawkish sentimentalism which is stealing over the present generation, and by a lessening of manliness is curtailing the good effects of increased enlightenment. The foolish of this world are often used to correct the wise; and actions brought about by a wrong sentimentalism may be in themselves right and good.

The question whether it is desirable that man should continue to inflict the pains of death, or pains without death, on other animals and, if so, within what limits, is one which must be argued out on its own merits alone, and the discussion of it will not be advanced by irrelevant considerations such as these on which we have dwelt.

There are two aspects of the inquiry—one from the side of man, the other from the side of the animal. Let us first consider the question from the point of view of the animal.

We have to determine the principles which govern or should govern the conduct of man toward animals. One broad principle may be briefly stated: Unless man destroys animals, animals would soon destroy man. Mr. Tennyson has told us: "Nature is one with rapine, a harm no preacher can heal;" and Mr. Darwin has shown that the lives of all living beings are shaped by "the struggle for existence." Man's life is a struggle for existence with his fellow-men, with living animals and plants, and with the lifeless forces of the universe. The very conditions of his existence lay upon him the burden, and in so doing give him the right, to use the world around him, the lives of animals included, to aid him in his strife. Imagine the results of forbidding man to take away the lives of animals. Suppose, for instance, the whole human race were to form itself into a Society for the Prevention of the Destruction of Tigers. How many generations would pass before "the last man" provided a tumultuous crowd of tigers with the last human meal?—possibly the indefatigable secretary of the Society sealing with his death his loyalty to the cause. Or, since tigers, like man, are carnivorous, and might therefore be supposed more worthy of death than herbiverous creatures, let us suppose the efforts of the Society to be directed toward the preservation of sheep. How many generations would pass before the face of the earth were covered with woolly flocks, and man were driven to lead a laborious, frugivorous, arboreal life in the treetops, or to earn a scanty subsistence on resuscitated *Pfahlbauten,* as being

the only places where the necessities of the sheep would permit him to dwell? Did the reader ever by chance descend, at early dawn, into the kitchen and watch the convulsive agonies of a writhing heap of cockroaches drowning in the watery trap set for them by the cook overnight? What a scene of unutterable woe is that when judged from the standpoint of the cockroach! But, if man were to deny himself the right of vivisection or vivipression over the vermin which infest his home and bed, what would come of it?

To be serious: man, if he is to live and prosper, *must* kill other animals. It is a duty laid upon him by the nature of things; a duty, and therefore a right. Self-preservation demands it. But what do we mean by self-preservation? Can we draw a line and say that he is justified in slaying an animal for this purpose and not for that? We can only do so by applying the test of whether the death of the animal is useful to him or not. Whenever or wherever the death of an animal is of advantage either to himself or to the human society of which he is a unit, he is justified in slaying that animal.

The success of the human race in the struggle for existence depends on man's being well-fed; man is therefore justified in slaying and eating a sheep. The success of the human race in the struggle for existence is dependent on knowledge being increased; man is therefore justified in slaying a frog or a rabbit, if it can be shown that human knowledge is thereby enlarged.

Death is in itself painful. It is only by special means that the pangs amid which the ties of life are loosened can be done away with. The slaughter of an animal is therefore of necessity painful, except in the special cases where means have been taken to do away with pain. In ninety-nine cases out of a hundred, when an animal is slaughtered by man, it is the death of the animal which benefits man, the pain itself which accompanies the death does him no good at all. While justified, therefore, in killing the animal, he is not justified in causing it pain. He is bound, in fact, to kill the animal in such a way as to cause as little pain as is consistent with his own interest. The death of a sheep in a butcher's slaughterhouse is painful; but men cannot therefore be said to do wrong in killing a sheep for food. They kill it with as little pain as is under the circumstances possible. They could not make the pain less, except by the introduction of elaborate and costly methods which would probably ruin the butcher or spoil the meat, or at least, in the present state of our knowledge and of the market, do damage to the interests of mankind. The death of an ox, again, is more painful than that of a sheep; but men do not therefore feel bound to live on mutton alone. They consider that the advantages of a mixed diet of beef and mutton justify them in inflicting that additional quantity of pain which is suffered whenever an ox is felled.

In short, this, under one aspect, is a selfish world. The struggle for existence is its guiding principle. If we believe that man is to govern the world, and he must either govern or succumb, then we must be prepared to use animals selfishly, if you please to call it so—to use animals for our advantage—to kill them when we have need of their deaths—to kill them with pain when the pain is for our benefit; and, inasmuch as the greater includes the less, to inflict pain without death where their pain does us good.† Our good is, in fact, the rule of our conduct toward animals. Whenever an animal is killed by man, or suffers pain at the hand of man, without benefit to man, or where the same benefit could be gained without the death or without the pain, then the death or the pain can be no longer justified. The man who inflicts them is a cruel man; he no longer does good, but harm, to humanity, and humanity ought to stop his hand.

I feel that I ought almost to apologize to the reader for having spent so much of his time over what are almost truisms; but so many absurd statements are continually being made, and so many whimsical ideas broached, that it seemed desirable to have a clear understanding concerning the principles which should guide our general conduct toward animals before discussing the special subject of vivisection.

We have now to inquire whether the deaths and pains which the word vivisection implies are, or have been, wrought for the benefit of mankind, inasmuch as they have led to knowledge and power which could not otherwise have been gained; or whether they have not been wrought for the benefit of mankind, inasmuch as they have not led to knowledge and power, or the power and knowledge might have been gained in some other way, or, being gained by many deaths and much pain, have been so small that mankind could well have done without them. I introduce the word death as well as pain, because, in spite of the etymology of the word, and the fact that vivisection suggests to the public mind pain only, and not death at all, the truth is, that in at least the great majority of cases vivisection does or ought to mean death only, and not pain at all. In the minds of those ignorant of physiology

† Some writers have urged that, while man is perfectly justified in *killing* any number of animals, he is not justified in causing *pain*. From the point of view of the animal this is simply a grotesque absurdity; from the point of view of man we shall have to speak of it later on.

—and they are foremost, if not alone, in blaming vivisection—much confusion has arisen from the different meanings attached to the words "life" and "living." I alluded to these in the beginning of this paper. To many such it is perhaps a revelation to learn that an animal may be kept alive—that is, with its heart in full working order, and its respiratory movements continuing with perfect regularity —for hours and hours after all signs of consciousness have disappeared. All operations performed on such an animal would come under the term vivisection; but, in the total absence of all signs of consciousness, it would be absurd to speak of pain. It would perhaps be a still greater revelation to such to learn that a frog, at a later stage in the series of events which we class together as death—when its brain and spinal cord have been instantaneously destroyed by an operation the pain of which may be said to be infinitesimal, and its heart removed at a time when feeling is impossible—may yet be made by proper means to kick and jump and move its body about in almost all possible ways. Any operation performed on the body of such a frog would by many be still called vivisection; but, to speak of such a mere mass of muscle and nerve as suffering pain, is about as truthful and rational as to say that it is cruel to cut down a tree, though a silly, ignorant looker-on might shriek when the leg moved, for about the same cause and with the same reason that the African grovels before his fetich.

Did the reader ever see a rabbit completely under the influence of chloral? Lying prostrate, with flaccid limbs, with head sunk back on the limp neck, motionless and still, at first sight it seems quite dead and gone. But a gentle heaving of the body, a rise and a fall every few seconds, tell you that it still breathes; and a finger placed on the chest may feel the quick throb of the still-beating heart. You pull it and pinch it; it does not move. You prick with a needle the exquisitely sensitive cornea of its eye; it makes no sign, save only perhaps a wink. You make a great cut through its skin with a sharp knife; it does not wince. You handle, and divide, and pinch nerves which, in ourselves, are full of feeling; it gives no sign of pain. Yet it is full of action. To the physiologist, its body, though poor in what the vulgar call life, is still the stage of manifold events and each event a problem, with a crowd of still-harder problems at its back. He therefore brings to bear on this breathing, pulsating, but otherwise quiescent, frame the instruments which are the tools of his research. He takes deft tracings of the ebb and flow of blood in the widening and narrowing vessels; he measures the time and the force of each throb of the heart, while by light galvanic touches he stirs this part or quiets that; he takes note of the rise and fall of the chest walls, as they quicken or grow slow, as they wax or wane, under this influence or that; he gathers the juice which pours from one or another gland; he divides this nerve, he stimulates that, and marks the result of each; he brings subtile poisons to bear on the whole frame, or on parts; and, having done what he wished to do, having obtained, in the shape of careful notes or delicate tracings, answers to the questions he wished to put, he finishes a painless death by the removal of all the blood from the body, or by any other means that best suit him at the time. I am not exaggerating when I say that this is at the present day one of the commonest forms of vivisectional experiment; this is what newspaper writers speak of as "torture," and, on the strength of it, accuse cultivated physiologists of barbaric cruelty.

A dog under chloroform or morphia may be brought to very nearly the same condition as a rabbit under chloral; but, as far as my experience goes, the same long duration of complete quiescence is maintained with greater difficulty. Dogs sometimes howl under chloroform or morphia when nothing is being done to them, and under circumstances in which they can be suffering no pain. At the moment when the cholorform begins to take effect upon them, when probably confused carnivorous visions chase through their brains, the howling is often excessive. Anyone who knows anything about the administration of chloroform to human beings is well aware how frequent cries and noises are in the stage of excitement, and how little dependence can be placed on them as signs of pain.

In a large number of cases, then, where anesthetics of one kind or another are used, vivisectional experiments cause no pain at all; and, as far as I know, in this country, at least, physiologists always use anesthetics where they can. They do so not only for the sake of the animal, but also for the sake of the experiment itself. Unless they are studying actual manifestations of feeling, pain, with all its consequences, is a disturbing element which must by all possible means be eliminated, if the experiment is to have its due value. The apparent lifelessness of the animal is the physiologist's opportunity; struggling limbs would utterly defeat his aims, and a sudden start might wreck his whole experiment. Chloroform and other anesthetics have immensely lessened human suffering, not only by simply diminishing pain, but even still more by putting it in the power of the surgeon to perform operations which he otherwise would not dare to attempt. In the same way they have powerfully aided the progress of physiology, by rendering pos-

sible new experiments, and by allowing the investigator to analyze securely phenomena which otherwise would, perhaps forever, have remained confused through the disturbances caused by pain.

There are some experiments, however, requiring vivisection, in which the use of chloral or other anesthetics is, for various reasons, inadmissible or undesirable. These form two classes. In the first and most numerous, the experiment is generally a short one, and quickly carried out, and the pain slight and transient. It is, of course, impossible for anyone to judge truly of the pain felt by any other body, and we may err in two ways in estimating the pain felt by animals. We may overestimate or underestimate it. Perhaps a rough but tolerably safe test of great pain or distress may be gained by noting whether the animal is willing to eat or not. When a rabbit, for instance, not previously starved, begins to munch carrots immediately after an operation, or even continues to munch during the greater part of the time the operation is being performed, it is only fair to conclude that the operation cannot be very painful. I may add that, in the experience of experimental physiologists, the skin of the dog and the rabbit—allowance being made for individual peculiarities—is not nearly so sensitive as the human skin.

The second class of experiments carried on without anesthetics—those entailing a considerable amount of pain—are not only by far the least numerous, *but must of necessity become less and less numerous as physiology advances.* The end which the physiologist has in view is to analyze the life of any being into its constituent factors. As his science advances, he becomes more and more able to disengage any one of these factors from the rest, and so to study it by itself. He can already, as we have seen, study the complicated phenomena of the circulation of the blood, of respiration, of various kinds of movement, quite apart from and independent of the presence of consciousness. As his knowledge widens and his means of research multiply, this power of analysis will grow more and more; and by and by, if physiology be allowed free scope for its development, there will come a day when the physiologist, in his experimental inquiries, will cause pain then, and then only, when pain is the actual object of his study. And that he will probably study best upon himself.

At the present day, the greatest amount of pain to animals is probably caused in experiments which perhaps hardly come under the title of vivisection—experiments in which the effects of starvation or of insufficient food, or the actions of poisons are being studied. These, however, lead to valuable results. The pain which is the greatest in amount, and the least worthy in object, is the pain which comes to animals whose bodies have been used as tests to ascertain the poisonous nature of some suspected material; but this is a matter of the witness box, not of physiology.

We may conclude, then, that physiologists are the cause to animals of much death, of a good deal of slight pain, and of some amount of severe pain. A very active physiologist will, for instance, in a year, be the means of bringing about, for the sake of science, as much death as a small village will, in a week, for the sake of its mouths and its fun, and will give rise to about as much pain as a not too enthusiastic sportsman in a short sporting season.

We have now to ask: What justification does he plead for this death and this pain? What good to mankind is thereby wrought which could not otherwise be gained?

His answer is that the science of physiology is thereby advanced; that our knowledge of the laws of life has, in the main, been won by experiments on living animals. He, of course, cannot, and no one can, tell the "might-have-been." Without any such experiments, physics and chemistry, aided by mathematics, might have synthetically resolved the problems of life (though even then it might be said that both physics and chemistry sprang from the older biologic lore; and not so long ago a common physiological preparation, the muscle and nerve of a frog, started a new epoch in physics); but, as a matter of history, experiments on living animals have been the steppingstones of physiological progress.

The great Vesalius, the founder of modern anatomy, turning his thoughts to the uses of the structures he had so well described, saw clearly that the problems opening up before him could be settled only by vivisection. In his great work, *De Corporis Humani Fabrica,* may be read the evidence, not only that he performed experiments on living animals, but that, had he not in so inscrutable a way forsaken the arduous pleasures of learning for the gossip of a court, those experiments would have led him up to and probably beyond the discovery which years afterward marked an epoch in physiology, and made the name of Harvey immortal. He, indeed, sowed the seed whose fruit Harvey reaped. The cornerstone of physiology, the doctrine of the circulation of the blood, was not built up without death and pain to animals. Today, it is true, much of the evidence touching the flow of blood may be shown on a dead body, yet the full proof cannot be given even now without an experiment on a living creature; and certainly Har-

vey's thoughts were guided by his study of the living, palpitating heart, and the motions of the living arteries, quite as much as by the suggestions coming from dead valves and veins.

After Harvey came Haller, whose keen intellect dispersed the misty notions of the spiritualists, and by the establishment of the doctrine of "irritability" laid the foundations of the true physiology of the nervous system: he too, in his work, wrought death and suffering on animals.

Another great step onward was made when Charles Bell and Magendie, by experiments on animals more painful than any of the present day, traced out the distinction between motor and sensory nerves; and yet another, when Marshall Hall and others demonstrated by vivisections the widespread occurrence and vast importance of reflex actions.

What was begun with death and pain has been carried forward by the same means. I assert deliberately that all our real knowledge of the physiology of the nervous system—compared with which all the rest of physiology, judged either from a practical or from a theoretical point of view, is a mere appendage—has been gained by experiment, that its fundamental truths have come to us through inquiries entailing more or less vivisection. By meditating over the differences in structure visible in the nervous systems of different animals, a shrewd observer might guess at the use of some particular part; but till verified by experiment, the guess would remain a guess; and experiment shows that such guesses may be entirely wrong. Where experiment has given a clue, careful observations have frequently thrown light on physiological problems. Without the experimental clue, the phenomena would ever have remained a hopeless puzzle, or have served to bolster up some baseless fancy. What disease, or what structure in what animal, could ever have made us acquainted with that "inhibitory" function of the pneumogastric nerve which the vivisectional experiment of Weber first detected? What a light that one experiment has thrown on the working of the nervous system! What disease could have told us that which we have learned from the experiments of Du Bois-Reymond and of Pflüger? Where would physiological science be now if the labors of Flourens, Brown-Séquard, Schiff, Vulpian, Goltz, Waller, and others were suddenly wiped away from the records of the past? Yet each of these names recalls long series of experiments, some of them painful in character, on living animals.

I repeat, take away from the physiology of the nervous system the backbone of experimental knowledge, and it would fall into a shapeless mass.

The chemistry of living beings, one would imagine at first thoughts, might be investigated without distressing the organisms which formed the subjects of research. The labors of Lavoisier and Priestley, who first made clear the chemistry of respiration, if they entailed no use of the knife, caused at times a no less painful suffocation; while the great advances which have been made in this branch of the study during the past quarter of a century, and are still being made, necessitate almost daily vivisection, in order that the gases of the blood may be studied in exactly the same condition as they are in the living body. Even still more bloody has been the path by following which we have gained the knowledge we now possess of the chemistry of digestion and nutrition. I have only to mention the names of Bidder, and Schmidt, and Bernard to call to the mind of the physiological student important results, nearly all reached through vivisection. The shifts and changes of the elements within our body are too subtile and complex to be divined from the results of the chemical laboratory; the physiologist has to search for them within the body, and to mark the compounds changing in the very spot where they change; otherwise all is guesswork.

Among the labors of the present generation, none perhaps have already more far-reaching results, none hold out more promise of fruit in the future, than those which bear on the influence of the nervous system over the circulation of the blood and over nutrition. The knowledge we are gradually acquiring of the subtile nervous bonds which bind together the unconscious members of the animal commonwealth, which make each part or organ at once the slave and guardian of every other, and which with cords of nervous sympathy draw each moiety of the body to work for the good of all, is putting a new aspect on physiology, and throwing many a gleam of light into the very darkest regions of the science. The words "inflammation" and "fever," bandied about of old as mystery-words, sounding much but signifying little—shuttlecocks tossed to and fro from one school of doctrinaire pathologists to another—now at last, through the labors of modern physiology, seem in a fair way of being understood. That understanding, when it is complete, will have been gained step by step through experiments on living animals, one of the first of which was Claude Bernard's research on vasomotor nerves.

There still remains the question, What good does physiology bring to mankind? Of the value of physiology as a not insignificant segment of the

circle of universal knowledge, nothing need be said; where saying aught is necessary, it would be useless. Nor need much be said concerning the practical value of physiology as a basis for the conduct of life. So long as men refuse to learn or to listen to physiology in order that they may the better use their bodies, it would be hopeless and useless to talk of the day when they may come to it for instruction how to form their minds and mould their natures. It will be enough for my present purpose to point out briefly the relations of physiology to the practical art of medicine.

These are twofold. In the first place, the medical profession is largely indebted to physiology on account of special discoveries and particular experimental researches. If we regard the profession simply as a body of men who possess, or should possess, a remedy for every disease, this may seem an exaggerated statement. Many of the remedies in use or in vogue at the present day have been discovered by chance, borrowed from ignorant savages, or lighted on by blind trials. Physiology can lay no claim to the introduction of opium or quinine. Where specific remedies have been suggested by physiological results or theories, it has not seldom happened that the remedies, though useful, have been given for a wrong reason, or have done good in a way which was not expected.

But if we look upon the medical profession as a body of men, cunning to detect the nature and to forecast the issues of the bodily ills under which we suffer, skillful in the use of means to avoid or to lessen those ills, rich in resources whereby pain is diminished and dangerous maladies artfully guided to a happy end, then we owe physiology many and great debts. Did the reader ever suffer, or witness others suffer, with subsequent relief, a severe surgical operation? If so, let him revere the name of John Hunter, the father of modern surgery. But Hunter was emphatically a physiologist; his surgery was but the carrying into practice of physiological ideas, many of which were got by experiments on living animals. Does the reader know that in all great surgical operations there are moments of imminent danger lest life steal away in gushes of blood from the divided vessels, danger now securely met by ligatures scientifically and deftly tied? Does he know that there was a time when the danger was imperfectly met by hot searing-irons and other rude means, and that the introduction of ligatures, with their proper application, is due to experiments, cruel experiments, if you like, on dogs and other dumb animals, experiments eminently physiological in their nature, about which much may be read in the book of *Jones on Hæmorrhage?* Even now, year by year, the scientific surgeon, by experiments on animals, is at once adding to physiological knowledge and bettering his treatment of wounded or diseased arteries. Has the reader seen anyone once stricken by paralysis, or bowed down by some nervous malady, yet afterward made whole and brought back to fair, if not vigorous, health? The advice which turned such a one toward recovery was based on knowledge originally drawn from the vivisectional experiments of physiologists, and made safe by matured experience. Or has he watched any dear friend fading away in that terrible malady diabetes, after rejoicing that for a season he seemed to be gathering strength and ceasing to fail, even if not regaining health? The only gleam of light into that mysterious disease which we possess came from the vivisectional researches of Claude Bernard on the formation of glycogen in the liver; and by judiciously acting upon the results of those researches the skillful physician can sometimes stay its ravages. He cannot cure it even now; and unless some empiric remedy be found by chance, will never cure it, until, by the death of many animals in the physiological laboratory, the mystery of the glycogenic function of the liver be cleared up.

But why need I go on adding one special benefit to another? They may all be summed up in one sentence, which embodies the whole relation of physiology to the medical profession.

The art of medicine is the science of physiology applied to detailed vital phenomena by the help of a wisdom which comes of enlightened experience, and an ingenuity which is born of practice. Were there not a single case on record in which physiology had given special and direct help to the cure of the sick, there would still remain the great truth that the ideas of physiology are the mother-ideas of medicine. The physiologist, unincumbered by the care of the sick, not weighted by the burden of desiring some immediate practical result, is the pioneer into the dark places of vital actions. The truths which he discovers in his laboratory pass over at once to the practitioner, busy in a constant struggle with the puzzling complexity of corporeal events: in his hands they are sifted, extended, and multiplied. The property of the physiologist alone, they might perhaps lie barren; used by the physician or surgeon, they soon bear fruit. The hint given by a physiologist of the past generation becomes a household word with the doctors of the present, and their records in turn offer rich stores of suggestive and corrective facts for the physiologists of the generation to come. Take away from

the practical art of medicine the theoretical truths of physiology, and you would have left a crowd of busy idlers in full strife over fantastic ideas. The reader has laughed with Molière over the follies of the doctrinaire physicians of times gone by. He has to thank experimental physiology that he has not the same follies to laugh over and to suffer from now. The so-called practical man is ever prone to entangle himself in and guide his conduct by baseless speculations. Such has been the case with medicine. The history of medicine in past centuries is largely occupied with the conflicts of contending schools of pathology—schools which arose from this or that master putting forward a fancy, or a fragment of truth, as the basis of all medical judgment. These have given place in the present century to a rational pathology, which knows no school and swears to the words of no master, but is slowly and surely unraveling, bit by bit, the many separate tangled knots of disease. They have given place because men have come to see that maladies can only be mastered through a scientific comprehension of the nature of disease; that pathology, the science of disease, being a part of, is inseparable from, physiology, the science of life; that the methods of both are the same, for in each a sagacious observation starts an inquiry, which a well-directed series of experiments brings to a successful end.

Many, if not most, of these experiments must be made on living beings. Hence it is that animals are killed and suffer pain, in order that physiological knowledge may be increased and disease made less.

Take away from the art of medicine all that with which physiology has enriched it, and the surgeon or the physician of today would be little better than a mystery-man, or a quack vender of chance-gotten drugs. Take out of the present system of physiology all that has been gained by experiments on living animals, and the whole structure would collapse, leaving nothing but a few isolated facts of human experience.

As far as we can see, what has been will be. The physiology of the future, if not hampered by any ignorant restraint, will, out of the death of animals, continue to press further and further into the mystery of—and year by year bring the physician, and not the physician only, but everyone, power to prolong, to strengthen, and to purify—the life of man. By no other way can man hope to gain this end. He is thereby justified for the death he causes and the pain he gives.

We have yet to consider this question in its other aspect; we have to examine not only the effects of vivisection as far as animals are concerned, but also its influence on man himself. Little, however, need be said. Necessary vivisection, we have shown, cannot be called cruel. The question of the necessity of any particular case can only be judged by the investigator himself. I content myself with asserting that any attempt to draw up, for the guidance of others, a general definition of necessary and unnecessary vivisection, must prove utterly futile. Only he who is making an inquiry knows his own needs. If he experiments recklessly and needlessly, he becomes cruel and, being cruel, will thereby be the worse. But, if he experiments carefully and heedfully, never causing pain where it could be avoided, never sacrificing a life without having in view some object, to attain which there seemed no other way, remembering that whoever "tortures" either dead or living nature carelessly will get no true response, there is no reason why his moral nature should suffer even ever so little tarnish. On the contrary, experience teaches us that earnest physiologists, who have killed animals in the single hope of gaining new truths or of making old ones plain, have grown more gentle and more careful the longer they worked and the more experiments they made.

The effects of vivisection on the moral nature of man may fairly be tested by experience. There are in this country several physiologists—myself among the number—who have for several years performed experiments on living animals. We have done repeatedly the things which a distinguished lady has seen fit to say "are best spoken of as nameless." I can confidently appeal to all who know us whether they have seen any deterioration in our moral nature as the result of our work; whether we are today less careful of giving pain than we were when we began to experiment; whether they can trace in us any lessening of that sympathy with dumb animals which all men should feel even in the very thickest of the struggle for existence.—*Macmillan's Magazine.*

# Physical Features of the Colorado Valley*

MAJOR J. W. POWELL

## Mountains and Valleys

THE topographical features of the valley of the Colorado, or the area drained by the Colorado River and its tributaries, are in many respects unique, as some of these features, perhaps, are not reproduced except to a very limited extent on any other portion of the surface of the globe. Mountains, hills, plateaus, plains, and valleys are here found, as elsewhere throughout the earth; but, in addition to these topographic elements in the scenic features of the region, we find buttes, outlying masses of stratified rocks, often of great altitude, not as dome-shaped or conical mounds, but usually having angular outlines; their sides are vertical walls, terraced or buttressed, and broken by deep, reëntering angles, and often naked of soil and vegetation.

Then we find lines of cliffs, abrupt escarpments of rock, of great length and great height, revealing the cut edges of strata swept away from the lower side. Thirdly, we find cañons, narrow gorges, scores or hundreds of miles in length, and hundreds or thousands of feet in depth, with walls of precipitous rocks.

In the arid region of the Western portion of the United States, there are certain tracts of country which have received the name of *Mauvaises Terres*, or Bad Lands. These are dreary wastes—naked hills, with rounded or conical forms, composed of sand, sandy clays, or fine fragments of shaly rocks, with steep slopes, and, yielding to the pressure of the foot, they are climbed only by the greatest toil, and it is a labor of no inconsiderable magnitude to penetrate or cross such a district of country. The steep hills are crowded together, and the waterways separating them are deep *arroyas* [*sic*]. Where the mud-rocks, or sandy clays and shales, of which the hills are composed, are interstratified with occasional harder beds, the slopes are terraced; and when these thinly bedded, though harder, rocks prevail, the outlines of the topography are changed, and present angular surfaces, and give rise to another type of topographic features, which I have denominated Alcove Lands.

The agencies and conditions under which all of these features have been formed deserve mention, and in this and following chapters I shall briefly discuss this subject, in a manner as free from technical terms as will be consistent with accurate description.

The discussion will by no means be exhaustive, and I hope hereafter to treat this subject in a more thorough manner. In view of these facts, I shall not attempt any logical classification of the elements of the topography, nor of the agencies and conditions under which they were produced; but, commencing at the north, at the initial point of the exploration, I shall take them up in geographic order, as we proceed down the river.

*Bad Lands and Alcove Lands North of the Uinta Mountains.* The area north of the Uinta Mountains embraced in the survey is but small. Through the middle of it runs Green River, in a deep, narrow valley, the sides or walls of which sometimes approach so near to each other, and are so precipitous, as to form a cañon.

The general surface of the country, on the north of this district, is about 1,000 feet above the river, with peaks here and there rising a few hundred feet higher; but south, toward the Uinta Mountains, this general surface, within a few miles of the river, gradually descends, and at the foot of the mountains we find a valley on either side, with a direction transverse to that of the course of Green River and parallel to the mountain range.

To the north, the waterways are all deeply eroded; the permanent streams have flood-plains of greater or lesser extent, but the channels of the

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wet-weather streams, i.e., those which are dry during the greater part of the year, are narrow and much broken by abrupt falls.

The rocks are the sediments of a dead lake and are quite variable in lithologic characteristics. We find thinly laminated shales, hard limestones, breaking with an angular fracture, crumbling Bad Land rocks, and homogeneous, heavily bedded sandstones.

The scenic features of the country are alike variable. On the cliffs about Green River City, towers and buttes are seen as you look from below, always regarded by the passing traveler as strange freaks of Nature. The limestones, interstratified with shales, give terraced and buttressed characteristics to the escarpments of the cañons and narrow valleys.

Immediately south of Bitter Creek, on the east side of Green River, there is a small district of country which we have called the Alcove Land. On the east it is drained by Little Bitter Creek, a dry gulch much of the year. This runs north into Bitter Creek, a permanent stream, which empties into the Green. The crest of this watershed is an irregular line, only two to four miles back from the river, but usually more than 1,000 feet above it, so that the waters have a rapid descent, and every shower-born rill has excavated a deep, narrow channel, and these narrow cañons are so close to each other as to be separated by walls of rock so steep, in most places, that they cannot be scaled, and many of these little cañons are so broken by falls as to be impassable in either direction.

The whole country is cut in this way into irregular, angular blocks, standing as buttresses, benches, and towers, about deep waterways and gloomy alcoves. . . .

To the west of Green River, and back some miles between Black's Fork and Henry's Fork, we have a region of buff-, chocolate-, and lead-colored Bad Lands. This Bad Land country differs from the Alcove Land, above mentioned, in that its outlines are everywhere beautifully rounded, as the rocks of which it is composed crumble quickly under atmospheric agencies, so that an exposure of solid rock is rarely seen; but we have the same abrupt descent of the streams, and the same elaborate system of water channels. Here we have loose, incoherent sandstones, shales, and clays, carved by a network of running waters into domes and cones, with flowing outlines. But still there is no vegetation, and the loose earth is naked. Occasionally, a thin stratum of harder rock will be found. Such strata will here and there form shelves or steps upon the sides of the mountains.

Traces of iron, and rarer minerals, are found in these beds, and, on exposure to the air, the chemical agencies give a greater variety of colors, so that the mountains and cones, and the strange forms of the Bad Lands, are elaborately and beautifully painted, not with the delicate tints of verdure, but with brilliant colors that are gorgeous when first seen, but which soon pall on the senses.

*The Uinta Mountains.* To the west of Green River stand the Wasatch Mountains, a system of peaks, tables, and elevated valleys having a northerly and southerly direction, nearly parallel to the river. The range known as the Uinta Mountains stands at right angles to the Wasatch, extending toward the east, and no definite line of division can

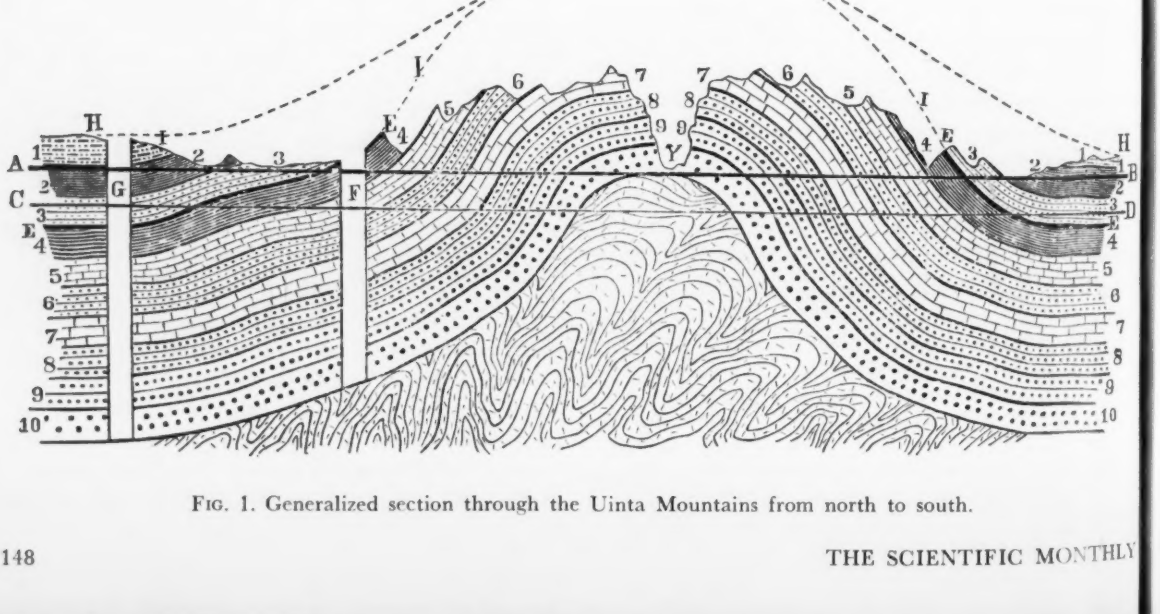



FIG. 1. Generalized section through the Uinta Mountains from north to south.

be noticed. The Wasatch is a great trunk, with a branch called the Uinta. Near the junction, the two ranges have about the same altitude, and the gulches of their summits are filled with perpetual snow; but, toward the east, the Uinta peaks are lower, gradually diminishing in altitude, until they are lost in low ridges and hills. Through this range Green River runs, and a series of cañons forms its channel.

To a person studying the physical geography of this country, without a knowledge of its geology, it would seem very strange that the river should cut through the mountains, when apparently it might have passed around them to the east through valleys, for there are such along the north side of the Uintas, extending to the east, where the mountains are degraded to hills; and, passing around these, there are other valleys, extending to the Green, on the south side of the range. Then why did the river run through the mountains?

The first explanation suggested is that it followed a previously formed fissure through the range; but very little examination will show that this explanation is unsatisfactory. The proof is abundant that the river cut its own channel; that the cañons are gorges of corrasion. Again, the question returns to us, Why did not the stream turn around this great obstruction, rather than pass through it? The answer is that the river had the right of way; in other words, it was running ere the mountains were formed: not before the rocks, of which the mountains are composed, were deposited, but before the formations were folded so as to make a mountain range.

The contracting or shriveling of the earth causes the rocks near the surface to wrinkle or fold, and such a fold was started athwart the course of the river. Had it been suddenly formed, it would have been an obstruction sufficient to turn the water in a new course to the east, beyond the extension of the wrinkle; but the emergence of the fold above the general surface of the country was little or no faster than the progress of the corrasion of the channel. We may say, then, that the river did not cut its way *down* through the mountains, from a height of many thousand feet above its present site; but, having an elevation differing but little, perhaps, from what it now has, as the fold was lifted it cleared away the obstruction by cutting a cañon, and the walls were thus elevated on either side. The river preserved its level, but mountains were lifted up, as the saw revolves on a fixed pivot while the log through which it cuts is moved along. The river was the saw which cut the mountains in two.

Recurring to the time before this wrinkle was formed, there were beds of sandstone, shale, and limestone, more than 24,000 feet in thickness, spread horizontally over a broad stretch of this country. Then the summit of the fold slowly emerged, until the lower beds of sandstone were lifted to the altitude at first occupied by the upper beds, and if these upper beds had not been carried away, they would now be found more than 24,000 feet above the river, and we should have a billow of sandstone, with its axis lying in an easterly and westerly direction, more than 100 miles in length, 50 miles in breadth, and over 24,000 feet higher than the present altitude of the river, gently rounded from its central line above to the foot of the slope on either side. But, as the rocks were lifted, rains fell upon them and gathered into streams, and the wash of the rains and the corrasion of the rivers cut the billow down almost as fast as it rose, so that the present altitude of these mountains marks only the difference between the elevation and the denudation.

It has been said that the elevation of the wrinkle was 24,000 feet, but it is probable that this is not the entire amount, for the present altitude of the river, above the sea, is nearly 6,000 feet, and when this folding began we have reason to believe that the general surface of this country was but slightly above that general standard of comparison.

Then there were downturned as well as upturned wrinkles, or, as the geologist would say, there were synclinal as well as anticlinal folds. Had there been no degradation of the fold, there would have been a bed of rock turned over its summit 24,000 feet above the present level of the river. Now that bed is gone from the mountains, yet it can be seen turned up on edge against the flanks of the mountains, dipping under the beds of rocks found still farther out from the range. Follow it down, and doubtless we could trace it to a depth much below the level of the sea. While the folds were forming, the upturned flexures were cut down, and the troughs in the downturned flexures were filled up, and we have more than 8,000 feet of these later sediments to the north of the Uinta Mountains.

It will thus be seen that the upheaval was not marked by a great convulsion, for the lifting of the rocks was so slow that the rains removed the sandstones almost as fast as they came up. The mountains were not thrust up as peaks, but a great block was slowly lifted, and from this the mountains were carved by the clouds—patient artists, who take what time may be necessary for their work.

We speak of mountains forming clouds about their tops: the clouds have formed the mountains. Lift a district of granite, or marble, into their re-

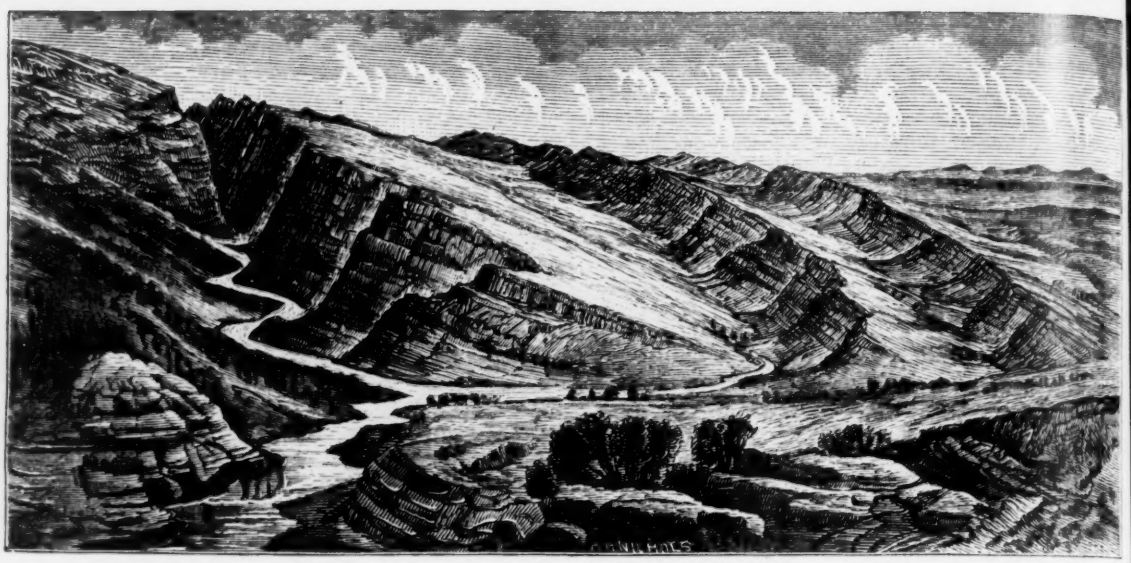


FIG. 2. Northern slope of the Uinta Mountains, showing Red Cañon and hogback, with intervening valleys.

gion, and they gather about it, and hurl their storms against it, beating the rocks into sands, and then they carry them out into the sea, carving out cañons, gulches, and valleys, and leaving plateaus and mountains embossed on the surface.

Instead of having a rounded billow, we have an irregular table, with beds dipping to the north, on the north side of the axis, and to south, on the south side, and in passing over the truncated fold we pass over their upturned edges.

Go out on the flank of the fold, and find the bed of rock which would form the summit of the great wrinkle, had there been no erosion, and there sink a shaft 24,000 feet, and you will be able to study a certain succession of beds of sandstones, shales, and limestones. Go two or three miles farther from the mountains, and sink a shaft; the first 8,000 feet or more will be through sandstones and shales, unlike those seen in the first section; then you will strike the summit of the first section. Continuing down for 24,000 feet, the first will be reproduced, stratum for stratum. Now start on either side of the fold, and cross to its centre; and you will pass over the same series of strata in the same order as you would in descending the first-mentioned shaft, and in the second also, below the upper 8,000 feet. Now pass again from the centre to the flank of the fold, in either direction, and you can study the same rocks in the same order as you would in ascending these shafts. It will thus be seen that in these truncated wrinkles we are enabled to study geological formations without descending into the depths of the earth.

Figure 1 has been constructed for the purpose of graphically expressing some of the important facts observed in the great Uinta Fold. In this, the beds are seen to turn up in a great flexure, and to be cut away above, the higher beds more than the lower; thus 4, 4–4, 4, has been cut away much more than 5, 5–5, 5; and 10, 10–10, 10 has suffered much less erosion than the beds above it. The only place where the water has carried it away is at *Y*, the bottom of the cañon.

In this diagram, the line *A–B* represents the lowest line of observation, as exhibited in the bed of the river. All below this line is theoretical. The line *C–D* represents the level of the sea. The stratum *E, E–E, E* was the last deposited antecedent to the commencement of the emergence of the summit of the fold. Had there been no erosion of the fold, the beds intervening between the broken line *I, I, I* (which is a continuation of the lines *E, E–E, E*) and the irregular line which represents the surface of the country, cutting the edges of the eroded beds, and passing through the lowest, No. 10, at *Y*, would still be found, but they have been carried away.

The diagram does not properly represent the entire amount of erosion, from the fact that the vertical scale is exaggerated, and the beds have been extended beyond their proper limits for the purpose of representing more clearly other facts of interest.

It will be seen that in passing along the line *A–B* (the bottom of the river channel), from the shaft *F* to the bottom of the cañon *Y*, we are able to observe the beds 4, 5, 6, 7, 8, 9, 10, in the same order that we would in descending the shaft *F*. The beds 1–1, 2–2 have been deposited since the emergence of the summit of the fold, and hence

never extended quite across it; yet the lower members of these beds doubtless at one time extended much farther up on the flanks of the fold. They have been cut away, however, as represented in the diagram. Let the lines *H, H–H, H* represent the limit of the continuation of these beds. In the shaft *G* these beds also are exposed above those seen in shaft *F*.

The altitude of the rocks above the line of observation (*A, B*) is exaggerated about five times. If they were reduced to one fifth, the proportion between the rocks seen in the various escarpments of these mountains, and those carried away below the broken lines, would be properly represented.

By sinking a shaft, only a little surface along the edge of the strata could be seen; but on the sides of the fold they are exposed for many miles, and often the top or bottom is cleared off for a great space, revealing even the ripple-marks of the ancient sea, or rounded impressions of raindrops which fell in that elder time; or the sands have buried shells and bones of ancient animals, and they are still encased in the rock; and even impressions of leaves that were buried in the mud can yet be seen in such a fine state of preservation that you can trace their delicate veins.

In speaking of the great upheaval of rocks from which the Uinta Mountains are carved, I have spoken of wrinkling and folding, as if the rocks were always flexed; but these displacements are sometimes attended with fractures, on one side of which the rocks are upheaved, or thrown down on the other. Such displacements are called faults. Faults like these are seen in many places in the Uinta Mountains; one great one, on the north side, the throw of which is nearly 20,000 feet, and many others of lesser magnitude are found.

In speaking of elevation and depression by faulting or folding, it must be understood that reference is made to a change of altitude in relation to the surface of the sea, so that upheaval or throw is only relative to this general standard of comparison. But during the geological ages represented in the folding and carving of the Uinta Mountains, it is possible the level of the sea itself has been changed by the shrinking of the earth, and a part, at least, of the apparent upheaval above mentioned may be accounted for by a depression of the formations in synclinal folds, and the letting down of broad areas of the earth's surface by lateral contraction exhibited in corrugation.

It has already been said that the cutting off of the fold has left the upturned edges of the formations exposed to view. Some of these beds are quite hard, others are composed of very soft material, so that there are alternating beds of harder and softer rocks running in an easterly and westerly direction, both on the north and south sides of the range. The soft rocks, yielding much more readily to atmospheric degradation, have been washed out in irregular valleys, between intervening ridges of harder rock, so that we have a series of nearly parallel valleys and also a series of intervening parallel ridges, and both valleys and ridges are approximately parallel to the range. But, as the great fold of the Uinta Mountains is greatly complicated by minor oblique and transverse flexures, while the general direction of these ridges is as described, they are turned back and forth from these lines in gentle or abrupt curves. These ridges are sometimes low mountain ranges.

So, if we approach these mountains from either direction, north or south, we first meet with ridges, or, as they are usually called in the Western country, hogbacks. In many places these are so steep as to form a complete barrier to progress.

Usually the slope away from the side of the mountain corresponds above with the dip of the rock, and is gentle or steep, as the dip is lesser or greater. The side of the hogback, next to the mountain, is composed of the cut edges of the strata, and varies greatly with the texture of the rocks; but usually it is steep or broken, sometimes buttressed, sometimes terraced, sometimes columned and fluted.

On the south side of the Yampa Plateau, near the head of Cliff Creek Valley, there is an abrupt, oblique flexure, on the side of the great fold, by which the rocks are turned up so as to stand vertically. In the rocks at this place there are two very hard conglomerates; the intervening strata are soft sandstones and marls, and have been carried away, and the conglomerates stand as vertical walls, 30 or 40 feet in thickness, 50–300 feet in height, and several miles in length, and between these is a broad avenue, or narrow valley, beset with ragged bowlders of conglomerate.

The drainage of these narrow valleys between the hogbacks is not always along their lengths, but the water is sometimes carried by channels crossing them and cutting through intervening ridges; hence there are numbers of transverse streams and wet-weather channels running across valleys and through ridges.

Now, if the great axis of the Uinta Fold were everywhere the summit of a watershed, we should find the streams heading along that irregular line running off to the flank of the fold on either side; but, as the fold is bisected by Green River, some of the minor watercourses, especially those near the

FIG. 3. Diaclinal valley.

river, and those near the centre of the fold, follow the strike of the rocks directly into that stream. On the north side, some head back near the summit of the fold and run to the north, crossing the hogbacks in a direction with the dip, and then turn, at the foot of the mountains, and run into the Green, where the waters take a general southerly direction. Others, again, head back on the hogbacks, or even beyond them, on the plains and the Bad Lands to the north, and cut quite through the hogbacks and mountains in a direction against the dip of the rocks, and empty into the Green. This is especially true where the river has its easterly and westerly direction through Brown's Park. On the other side of the range, streams head high up in the mountains, and cut directly or obliquely against the upturned edges of the strata, and run in a general direction with the dip of the strata until they reach the long valleys between hogbacks; then down these valleys they turn, sometimes cutting through intervening ridges, until they find their way into the Green, where they are turned to the south.

It will thus be seen that the relation of the direction of the streams to the dip of the rocks is very complex, and, for convenience of description, I have elsewhere classified these valleys, on the basis of these relations, in the following order:

ORDER 1.—*Transverse* valleys, having a direction at right angles to the strike.

ORDER 2.—*Longitudinal* valleys, having a direction the same as the strike.

Of the first order, three varieties are noticed:

*a, diaclinal,* those which pass through a fold (Fig. 3).

*b, cataclinal,* valleys that run in the direction of the dip (Fig. 4).

*c, anaclinal,* valleys that run against the dip of the beds (Fig. 7).

Of the second order, we have, also, three varieties:

*A, anticlinal* valleys, which follow anticlinal axes (Fig. 5).

*B, synclinal* valleys, which follow synclinal axes (Fig. 6).

*C, monoclinal* valleys, which run in the direction of the strike between the axes of the fold—one side of the valley formed of the summits of the beds, the other composed of the cut edges of the formation (Fig. 8).

Many of the valleys are thus simple in their relations to the folds; but, as we may have two systems of displacements, a valley may belong to one class, in relation to one fold, and to another in its relation to a second. Such we designate as *complex* valleys.

Again, a valley may belong to one class in one

FIG. 4. A cataclinal valley.

part of its course and to another elsewhere in its course. Such we designate as *compound* valleys. It will be further noticed that valleys may have many branches, but, in relegating a valley to its class, we consider only the stem of the valley proper and not its branches.

A great diversity in the features of all these valleys is observed. Most of these modifications are due to three principal causes: First, a greater or lesser inclination of the rocks. Second, the texture of the beds—that is, their greater or lesser degree of heterogeneity. The third class of modifying influences is found in the eruptive beds.

The last-mentioned agencies are not found in the region under immediate discussion.

The explanations of the cañons of Green River will assist us in understanding the origin of the lateral valleys and cañons. The streams were there before the mountains were made; that is, the streams carved out the valleys, and left the mountains. The direction of the streams is indubitable evidence that the elevation of the fold was so slow as not to divert the streams, although the total amount of elevation was many thousands of feet. Had the fold been lifted more rapidly than the principal streams could have cut their channels, Green River would have been turned about it, and all the smaller streams and waterways would have been cataclinal.

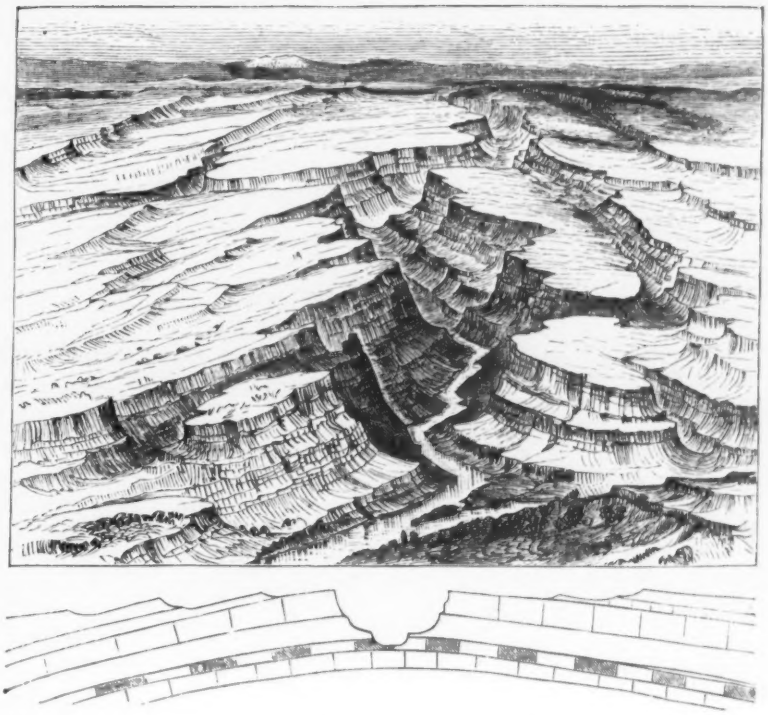
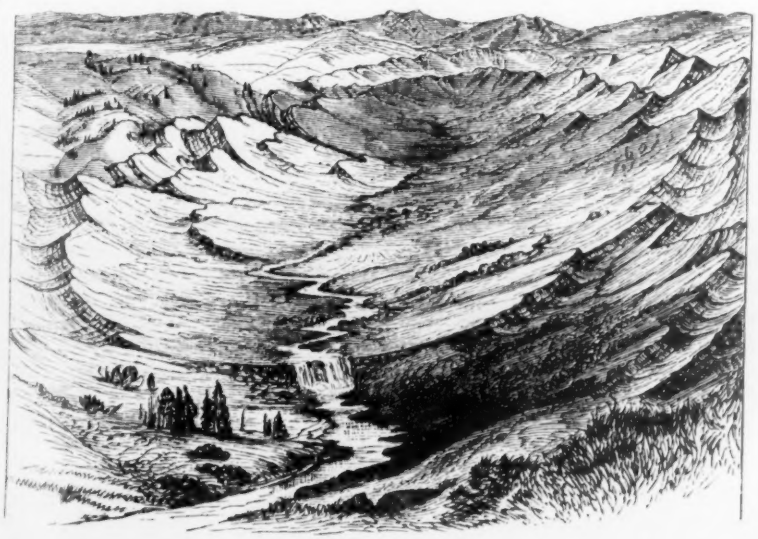


FIG. 5. An anticlinal valley, with section.

Thus it is that the study of the structural characteristics of the valleys and cañons teaches us, in no obscure way, the relation between the progress of upheaval and that of erosion and corrasion, showing that these latter were *pari passu* with the former, and that the agencies of Nature produce great results—results no less than the carving of a mountain range out of a much larger block lifted from beneath the sea; not by an extravagant and violent use of power, but by the slow agencies which may be observed generally throughout the world, still acting in the same slow, patient manner.

There are yet some interesting facts to be observed concerning these interhogback valleys. Their floors are usually lower than the general surface farther away from the mountains. There seem to be two causes for this: The great fold having been lifted and truncated prior to the exposure of the rocks farther away from the mountains, its strata present their edges, instead of their upper surfaces, to the down-falling rain,

FIG. 6. A synclinal valley.



and the softer beds are not so well shielded by the harder. Erosion hence progresses more rapidly than where the beds are approximately horizontal.

Again, the mountains, with peaks among the clouds, condense their moisture, and a greater quantity of rain falls on them, or in their vicinity. The region of country adjacent to the mountains receives a portion of this extra rainfall, so that this dynamic agency increases from the plains to the summits of the mountains, probably in some direct ratio. This increase of the eroding agency, and the greater exposure of the soft beds, probably account for the fact that the lowest country is at the foot of the mountains.

There is a limit to the effect of these conditions, for it should be observed that no valley can be eroded below the level of the principal stream, which carries away the products of its surface degradation; and where the floor of such a valley has been cut down nearly to the level of such a stream, it receives the debris of the adjacent cliffs and mountains, and in this way the rocks composing the floor are usually masked, to a greater or lesser extent. The same topographic facts, under like conditions, are found on the eastern slope of the Rocky Mountains, in Colorado Territory, and the valleys which run into the South Platte from the south, between the hogbacks, are lower than the mesas and plateaus farther away from the mountains, but not lower than the flood-plain of the river.

I have endeavored above to explain the relation of the valleys of the Uinta Mountains to the stratigraphy, or structural geology, of the region, and, further, to state the conclusion reached, that the drainage was established antecedent to the corrugation or displacement of the beds by faulting and folding. I propose to call such valleys, including the orders and varieties before mentioned, *antecedent* valleys.

FIG. 7. An anaclinal valley.

FIG. 8. Monoclinal valley.

In other parts of the mountain region of the West, valleys are found having directions dependent on corrugation. I propose to call these *consequent* valleys. Such valleys have been observed only in limited areas, and have not been thoroughly studied, and I omit further discussion of them.

In many cases, there can be no doubt that the present courses of the streams were determined by conditions not found in the rocks through which the channels are now carved, but that the beds in which the streams had their origin, when the district last appeared above the level of the sea, have been swept away. I propose to call such *superimposed* valleys. Thus the valleys under consideration, if classified on the basis of their relation to the rocks in which they originated, would be called *consequent* valleys; but, if classified on the basis of their relation to the rocks in which they are now found, would be called *superimposed* valleys.

(*Parts II and III of this article appeared in later issues of the same volume of* The Popular Science Monthly.)

# Food Poisoning*

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WITHIN the past fifteen or twenty years cases of poisoning with foods of various kinds have apparently become quite numerous. This increase in the number of instances of this kind has been both apparent and real. In the first place, it is only within recent years that it has been recognized that foods ordinarily harmless may become most powerful poisons. In the second place, the more extensive use of preserved foods of various kinds has led to an actual increase in the number of outbreaks of food poisoning.

The harmful effects of foods may be due to any of the following causes:

1. Certain poisonous fungi may infect grains. This is the cause of epidemics of poisoning with ergotized bread, which formerly prevailed during certain seasons throughout the greater part of continental Europe, but which are now practically limited to southern Russia and Spain. In this country ergotism is practically unknown, except as a result of the criminal use of the drug ergot. However, a few herds of cattle in Kansas and Nebraska have been quite extensively affected with this disease.

2. Plants and animals may feed upon substances that are not harmful to them, but which may seriously affect man on account of his greater susceptibility. It is a well-known fact that hogs may eat large quantities of arsenic or antimony without harm to themselves, and thus render their flesh unfit for food for man. It is believed that birds that feed upon the mountain laurel furnish a food poisonous to man.

3. During periods of the physiological activity of certain glands in some of the lower animals the flesh becomes harmful to man. Some species of fish are poisonous during the spawning season.

4. Both animal and vegetable foods may become infected with the specific germs of disease and serve as the carriers of the infection to man. Instances of the distribution of typhoid fever by the milkman are illustrations of this.

5. Animals may be infected with specific diseases, which may be transmitted to man in the meat or milk. This is one of the means by which tuberculosis is spread.

6. Certain nonspecific, poison-producing germs may find their way into foods of various kinds, and may by their growth produce chemical poisons either before or after the food has been eaten. This is the most common form of food poisoning known in this country.

We will briefly discuss some foods most likely to prove harmful to man.

MUSSEL POISONING.—It has long been known that this bivalve is occasionally poisonous. Three forms of mussel poisoning are recognized. The first, known as *Mytilotoxismus gastricus*, is accompanied by symptoms practically identical with those of cholera morbus. At first there is nausea, followed by vomiting, which may continue for hours. In severe cases the walls of the stomach are so seriously altered that the vomited matter contains considerable quantities of blood. Vomiting is usually accompanied by severe and painful purging. The heart may be markedly affected, and death may result from failure of this organ. Examination after death from this cause shows the stomach and small intestines to be highly inflamed.

The second form of mussel poisoning is known as *Mytilotoxismus exanthematicus* on account of visible changes in the skin. At first there is a sensation of heat, usually beginning in the eyelids, then spreading to the face, and finally extending over the whole body. This sensation is followed by an eruption, which is accompanied by intolerable itching. In severe cases the breathing becomes labored, the face grows livid, consciousness is lost, and death may result within two or three days.

The most frequently observed form of mussel poisoning is that designated as *Mytilotoxismus paralyticus*. As early as 1827 Combe reported his observations upon thirty persons who had suffered from this kind of mussel poisoning. The first symptoms, as a rule, appeared within two hours after eating the poisonous food. Some suffered from nausea and vomiting, but these were not constant or lasting

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symptoms. All complained of a prickly feeling in the hands, heat and constriction of the throat, difficulty of swallowing and speaking, numbness about the mouth, gradually extending over the face and to the arms, with great debility of the limbs. Most of the sufferers were unable to stand; the action of the heart was feeble, and the face grew pale and expressed much anxiety. Two of the thirty cases terminated fatally. Post-mortem examination showed no abnormality.

Many opinions have been expressed concerning the nature of harmful mussels. Until quite recently it was a common belief that certain species are constantly toxic. Virchow has attempted to describe the dangerous variety of mussels, stating that it has a brighter shell, sweeter, more penetrating, bouillonlike odor than the edible kind, and that the flesh of the poisonous mussel is yellow; the water in which they are boiled becomes bluish.

However, this belief in a poisonous species is now admitted to be erroneous. At one time it was suggested that mussels became hurtful by absorbing the copper from the bottoms of vessels, but Christison made an analysis of the mussels that poisoned the men mentioned by Combe, with negative results, and also pointed out the fact that the symptoms were not those of poisoning with copper. Some have held that the ill effects were due wholly to idiosyncrasies in the consumers, but cats and dogs are affected in the same way as men are. It has also been believed that all mussels are poisonous during the period of reproduction. This theory is the basis of the popular superstition that shellfish should not be eaten during the months in the name of which the letter "r" does not occur. At one time this popular idea took the form of a legal enactment in France forbidding the sale of shellfish from May 1 to September 1. This widespread idea has a grain of truth in it, inasmuch as decomposition is more likely to alter food injuriously during the summer months. Poisoning with mussels may occur at any time of the year, however.

It has been pretty well demonstrated that the first two forms of mussel poisoning mentioned above are due to putrefactive processes, whereas the paralytic manifestations seen in other cases are due to a poison isolated a few years ago by Brieger, and named by him mytilotoxin. Any mussel may acquire this poison when it lives in filthy water. Indeed, it has been shown experimentally that edible mussels may become harmful when left for fourteen days or longer in filthy water; on the other hand, poisonous mussels may become harmless if kept four weeks or longer in clear water. This is true not only of mussels, but of oysters as well. Some years ago, many cases of poisoning from oysters were reported at Havre. The oysters had been taken from a bed near the outlet of a drain from a public water closet. Both oysters and mussels may harbor the typhoid bacillus, and may act as carriers of this germ to man.

There should be most stringent police regulations against the sale of all kinds of mollusks, and all fish as well, taken from filthy waters. Certainly one should avoid shellfish from impure waters, and it is not too much to insist that those offered for food should be washed in clean water. All forms of clam and oyster broth should be avoided when it has stood even for a few hours at summer heat. These preparations very quickly become infected with bacteria, which develop most potent poisons.

Fish Poisoning.—Some fish are supplied with poisonous glands, by means of which they secure their prey and protect themselves from their enemies. The "dragon weaver," or "sea weaver" (*Trachinus draco*), is one of the best known of these fish. There are numerous varieties widely distributed in salt waters. The poisonous spine is attached partly to the maxilla and partly to the gill cover at its base. This spine is connected with a poisonous gland; the spine itself is grooved and covered with a thin membrane, which converts the grooves into canals. When the point enters another animal, the membrane is stripped back and the poison enters the wound. Men sometimes wound their feet with the barbs of this fish while bathing. It also occasionally happen that a fisherman pricks his fingers with one of these barbs. The most poisonous variety of this fish known is found in the Mediterranean Sea. Wounds produced by these animals sometimes cause death. In *Synanceia brachio* there are in the dorsal fin thirteen barbs, each connected with two poison reservoirs. The secretion from these glands is clear, bluish in color, and acid in reaction, and when introduced beneath the skin causes local gangrene and, if in sufficient quantity, general paralysis. In *Plotosus lineatus* there is a powerful barb in front of the ventral fin, and the poison is not discharged unless the end of the barb is broken. The most poisonous variety of this fish is found only in tropical waters. In *Scorpoena scrofa* and other species of this family there are poison glands connected with the barbs in the dorsal and in some varieties in the caudal fin.

A disease known as *kakke* was a few years ago quite prevalent in Japan and other countries along the eastern coast of Asia. With the opening up of Japan to the civilized world, the study of this disease by scientific methods was undertaken by the

observant and intelligent natives, who acquired their medical training in Europe and America. In Tokio the disease generally appears in May, reaches its greatest prevalence in August, and gradually disappears in September and October. The researches of Miura and others have fairly well demonstrated that this disease is due to the eating of fish belonging to the family of *Scombridae*. There are other kinds of fish in Japanese waters that undoubtedly are poisonous. This is true of the *tetrodon*, of which, according to Remey, there are twelve species whose ovaries are poisonous. Dogs fed upon these organs soon suffered from salivation, vomiting, and convulsive muscular contractions. When some of the fluid obtained by rubbing the ovaries in a mortar was injected subcutaneously in dogs, the symptoms were much more severe, and death resulted. Tahara states that he has isolated from the roe of the tetrodon two poisons, one of which is a crystalline base, and the other a white, waxy body. From 1885 to 1892, inclusive, 933 cases of poisoning with this fish were reported in Tokio, with a mortality of 72 per cent.

Fish poisoning is quite frequently observed in the West Indies, where the complex of symptoms is designated by the Spanish term *siguatera*. It is believed by the natives that the poisonous properties of the fish are due to the fact that they feed upon decomposing medusæ and corals. In certain localities it is stated that all fish caught off certain coral reefs are unfit for food. All statements concerning the origin and nature of the poison in these fish are mere assumptions, however, since no scientific work has been done. Whatever the source of the poison may be, it is quite powerful, and death not infrequently results. The symptoms are those of gastrointestinal irritation followed by collapse.

In Russia fish poisoning sometimes causes severe and widespread epidemics. The government has offered a large reward for anyone who will positively determine the cause of the fish being poisonous and suggest successful means of preventing these outbreaks. Schmidt, after studying several of these epidemics, comes to the following conclusions: (*a*) The harmful effects are not due to putrefactive processes. (*b*) Fish poisoning in Russia is always due to the eating of some member of the sturgeon tribe. (*c*) The ill effects are not due to the method of catching the fish, the use of salt, or to imperfections in the methods of preservation. (*d*) The deleterious substance is not uniformly distributed through the fish, but is confined to certain parts. (*e*) The poisonous portions are not distinguishable from the nonpoisonous, either macroscopically or microscopically. (*f*) When the fish is cooked it may be eaten without harm. (*g*) The poison is an animal alkaloid produced most probably by bacteria that cause an infectious disease in fish during life.

The conclusion reached by Schmidt is confirmed by the researches of Madame Sieber, who found a poisonous bacillus in fish which had caused an epidemic.

In the United States fish poisoning is most frequently due to decomposition in canned fish. The most prominent symptoms are nausea, vomiting, and purging. Sometimes there is a scarlatinous rash, which may cover the whole body. The writer has studied two outbreaks of this kind of fish poisoning. In both instances canned salmon was the cause of the trouble. Although a discussion of the treatment of food poisoning is foreign to this paper, the writer must call attention to the danger in the administration of opiates in cases of poisoning with canned fish. Vomiting and purging are efforts on the part of Nature to remove the poison and should be assisted by the stomach tube and by irrigation of the colon. In one of the cases seen by the writer, large doses of morphine had been administered in order to check the vomiting and purging and to relieve the pain; in this case death resulted. The danger of arresting the elimination of the poison in all cases of food poisoning cannot be too emphatically pointed out.

MEAT POISONING.—The diseases most frequently transmitted from the lower animals to man by the consumption of the flesh or milk of the former by the latter are tuberculosis, anthrax, symptomatic anthrax, pleuropneumonia, trichinosis, mucous diarrhœa, and actinomycosis. It hardly comes within the scope of this article to discuss in detail the transmission of these diseases from the lower animals to man. However, the writer must be allowed to offer a few opinions concerning some moot questions pertaining to the consumption of the flesh of tuberculous animals. Some hold that it is sufficient to condemn the diseased part of the tuberculous cow, and that the remainder may be eaten with perfect safety. Others teach that "total seizure" and destruction of the entire carcass by the health authorities are desirable. Experiments consisting of the inoculation of guinea pigs with the meat and meat juices of tuberculous animals have given different results to several investigators. To one who has seen tuberculous animals slaughtered, these differences in opinion and in experimental results are easily explainable. The tuberculous invasion may be confined to a single gland, and this may

occur in a portion of the carcass not ordinarily eaten; on the other hand, the invasion may be much more extensive, and the muscles may be involved. The tuberculous portion may consist of hard nodules that do not break down and contaminate other tissues in the process of removal, but the writer has seen a tuberculous abscess in the liver holding nearly a pint of broken-down infected matter ruptured or cut in removing this organ, and its contents spread over the greater part of the carcass. This explains why one investigator succeeds in inducing tuberculosis in guinea pigs by introducing small bits of meat from a tuberculous cow into the abdominal cavity, and another equally skillful bacteriologist follows the same details and fails to get positive results. No one desires to eat any portion of a tuberculous animal, and the only safety lies in "total seizure" and destruction. That the milk from tuberculous cows, even when the udder is not involved, may contain the specific bacillus has been demonstrated experimentally. The writer has suggested that everyone selling milk should be licensed, and the granting of a license should be dependent upon the application of the tuberculin test to every cow from which milk is sold. The frequency with which tuberculosis is transmitted to children through milk should justify this action.

That a profuse diarrhœa may render the flesh of an animal unfit food for man was demonstrated by the cases studied by Gärtner. In this instance the cow was observed to have a profuse diarrhœa for two days before she was slaughtered. Both the raw and cooked meat from this animal poisoned the persons who ate it. Medical literature contains the records of many cases of meat poisoning due to the eating of the flesh of cows slaughtered while suffering from puerperal fever. It has been found that the flesh of animals dead of symptomatic anthrax may retain its infection after having been preserved in a dry state for ten years.

One of the most frequently observed forms of meat poisoning is that due to the eating of decomposed sausage. Sausage poisoning, known as *botulismus*, is most common in parts of Germany. Germans who have brought to the United States their methods of preparing sausage occasionally suffer from this form of poisoning. The writer had occasion two years ago to investigate six cases of this kind, two of which proved fatal. The sausage meat had been placed in uncooked sections of the intestines and alternately frozen and thawed and then eaten raw. In this instance the meat was infected with a highly virulent bacillus, which resembled very closely the *Bacterium coli*.

In England, Ballard has reported numerous epidemics of meat poisoning, in most of which the meat had become infected with some nonspecific, poison-producing germ. In 1894 the writer was called upon to investigate cases of poisoning due to the eating of pressed chicken. The chickens were killed Tuesday afternoon and left hanging in a market room at ordinary temperature until Wednesday forenoon, when they were drawn and carried to a restaurant and here left in a warm room until Thursday, when they were cooked (not thoroughly), pressed, and served at a banquet in which nearly two hundred men participated. All ate of the chicken, and were more or less seriously poisoned. The meat contained a slender bacillus, which was fatal to white rats, guinea pigs, dogs, and rabbits.

Ermengem states that since 1867 there have been reported 112 epidemics of meat poisoning, in which 6,000 persons have been affected. In 103 of these outbreaks the meat came from diseased animals, whereas in only five was there any evidence that putrefactive changes in the meat had taken place. My experience convinces me that in this country meat poisoning frequently results from putrefactive changes.

Instances of poisoning from the eating of canned meats have become quite common. Although it may be possible that in some instances the ill effects result from metallic poisoning, in a great majority of cases the poisonous substances are formed by putrefactive changes. In many cases it is probable that decomposition begins after the can has been opened by the consumer; in others the canning is imperfectly done, and putrefaction is far advanced before the food reaches the consumer. In still other instances the meat may have been taken from diseased animals, or it may have undergone putrefactive changes before the canning. It should always be remembered that canned meat is especially liable to putrefactive changes after the can has been opened, and when the contents of the open can are not consumed at once the remainder should be kept in a cold place or should be thrown away. People are especially careless on this point. Although everyone knows that fresh meat should be kept in a cold place during the summer, an open can of meat is often allowed to stand at summer temperature and its contents eaten hours after the can has been opened. This is not safe, and has caused several outbreaks of meat poisoning that have come under the observation of the writer.

Milk Poisoning.—In discussing this form of food poisoning we will exclude any consideration of the distribution of the specific infectious diseases

through milk as the carrier of the infection, and will confine ourselves to that form of milk poisoning which is due to infection with nonspecific, poison-producing germs. Infants are highly susceptible to the action of the galactotoxicons (milk poisons). There can no longer be any doubt that these poisons are largely responsible for much of the infantile mortality, which is alarmingly high in all parts of the world. It has been positively shown that the summer diarrhœa of infancy is due to milk poisoning. The diarrhœas prevalent among infants during the summer months are not due to a specific germ, but there are many bacteria that grow rapidly in milk and form poisons which induce vomiting and purging, and may cause death. These diseases occur almost exclusively among children artificially fed. It is true that there are differences in chemical composition between the milk of woman and that of the cow, but these variations in percentage of proteids, fats, and carbohydrates are of less importance than the infection of milk with harmful bacteria. The child that takes its food exclusively from the breast of a healthy mother obtains a food that is free from poisonous bacteria, whereas the bottle-fed child may take into its body with its food a great number and variety of germs, some of which may be quite deadly in their effects. The diarrhœas of infancy are practically confined to the hot months, because a high temperature is essential to the growth and wide distribution of the poison-producing bacteria. Furthermore, during the summertime these bacteria grow abundantly in all kinds of filth. Within recent years the medical profession has so urgently called attention to the danger of infected milk that there has been a great improvement in the care of this article of diet, but that there is yet room for more scientific and thorough work in this direction must be granted. The sterilization and pasteurization of milk have doubtless saved the lives of many children, but every intelligent physician knows that even the most careful mother or nurse often fails to secure a milk that is altogether safe.

It is true that milk often contains germs the spores of which are not destroyed by the ordinary methods of sterilization and pasteurization. These germs are not the most dangerous ones found in milk, however. Moreover, every mother and nurse should remember that in the preparation of sterilized milk for the child it is not only necessary to heat the milk, but, after it has been heated to a temperature sufficiently high and sufficiently prolonged, the milk must subsequently be kept at a low temperature until the child is ready to take it, when it may be warmed. It should be borne in mind that the subsequent cooling of the milk and keeping it at a low temperature are necessary features in the preparation of it as a food for the infant.

Cheese Poisoning.—Under this heading we shall include the ill effects that may follow the eating of not only cheese but other milk products, such as ice cream, cream custard, cream puffs, etc. Any poison formed in milk may exist in the various milk products, and it is impossible to draw any sharp line of distinction between milk poisoning and cheese poisoning. However, the distinction is greater than is at first apparent. Under the head of milk poisoning we have called especial attention to those substances formed in milk to which children are particularly susceptible; in cheese and other milk products there are formed poisonous substances against which age does not give immunity. Since milk is practically the sole food during the first year or eighteen months of life, the effect of its poisons upon infants is of the greatest importance; on the other hand, milk products are seldom taken by the infant, but are frequent articles of diet in afterlife.

In 1884 the writer succeeded in isolating from poisonous cheese a highly active basic substance, to which he gave the name *tyrotoxicon*. The symptoms produced by this poison are quite marked, but differ in degree according to the amount of the poison taken. At first there is dryness of the mouth, followed by constriction of the fauces, then nausea, vomiting, and purging. The first vomited matter consists of food, then it becomes watery and is frequently stained with blood. The stools are at first semisolid, and then are watery and serous. The heart is depressed, the pulse becomes weak and irregular, and in severe cases the face appears cyanotic. There may be dilatation of the pupil, but this is not seen in all. The most dangerous cases are those in which the vomiting is slight and soon ceases altogether, and the bowels are constipated from the beginning. Such cases as these require prompt and energetic treatment. The stomach and bowels should be thoroughly irrigated in order to remove the poison, and the action of the heart must be sustained.

At one time the writer believed that tyrotoxicon was the active agent in all samples of poisonous cheese, but more extended experimentation has convinced him that this is not the case. Indeed, this poison is rarely found, whereas the number of poisons in harmful cheese is no doubt considerable. There are numerous poisonous albumins found in cheese and other milk products. All these are gastro-intestinal irritants, but they differ considerably in other respects.

In 1895 the writer and Perkins made a prolonged study of a bacillus found in cheese which had poisoned fifty people. Chemically, the poison produced by this germ is distinguished from tyrotoxicon by the fact that it is not removed from alkaline solution with ether. Physiologically, the new poison has a more pronounced effect on the heart, in which it resembles muscarin or neurin more closely than it does tyrotoxicon. Pathologically, the two poisons are unlike, inasmuch as the new poison induces marked congestion of the tissues about the point of injection when used upon animals hypodermically. Furthermore, the intestinal constrictions which are so uniformly observed in animals poisoned by tyrotoxicon were not once seen in our work with this new poison, although it was carefully looked for in all our experiments.

In 1898 the writer, with McClymonds, examined samples of cheese from more than sixty manufacturers in this country and in Europe. In all samples of ordinary American green cheese poisonous germs were found in greater or less abundance. These germs resemble very closely the colon bacillus, and most likely their presence in the milk is to be accounted for by contamination with bits of fecal matter from the cow. It is more than probable that the manufacture of cheese is yet in its infancy, and we need someone to do for this industry what Pasteur did for the manufacture of beer. At present the flavor of a given cheese depends upon the bacteria and molds which accidentally get into it. The time will probably come when all milk used for the manufacture of cheese will be sterilized, and then selected molds and bacteria will be sown in it. In this way the flavor and value of a cheese will be determined with scientific accuracy, and will not be left to accident.

Canned Foods.—As has been stated, the increased consumption of preserved foods is accountable for a great proportion of the cases of food poisoning. The preparation of canned foods involves the application of scientific principles, and since this work is done by men wholly ignorant of science it is quite remarkable that harmful effects do not manifest themselves more frequently than they do. Every can of food that is not thoroughly sterilized may become a source of danger to health and even to life. It may be of interest for us to study briefly the methods ordinarily resorted to in the preparation of canned foods. With most substances the food is cooked before being put into the can. This is especially true of meats of various kinds. Thorough cooking necessarily leads to the complete sterilization of the food; but after this it must be transferred to the can, and the can must be properly closed. With the handling necessary in canning the food, germs are likely to be introduced. Moreover, it is possible that the preliminary cooking is not thoroughly done and complete sterilization is not reached. The empty can should be sterilized. If one wishes to understand the *modus operandi* of canning foods, let him take up a round can of any fruit, vegetable, or meat and examine the bottom of the can, which is in reality the top during the process of canning and until the label is put on. The food is introduced through the circular opening in this end, now closed by a piece which can be seen to be soldered on. After the food has been introduced through this opening the can and contents are heated either in a water bath or by means of steam. The opening through which the food was introduced is now closed by a circular cap of suitable size, which is soldered in position.

This cap has near its center a "prick-hole" through which the steam continues to escape. This prick-hole is then closed with solder, and the closed can again heated in the water bath or with steam. If the can "blows" (if the ends of the can become convex) during this last heating, the prick-hole is again punctured and the heated air allowed to escape, after which the prick-hole is again closed. Cans thus prepared should be allowed to stand in a warm chamber for four or five days. If the contents have not been thoroughly sterilized gases will be evolved during this time, or the can will blow, and the contents should be discarded. Unscrupulous manufacturers take cans which have blown, prick them to allow the escape of the contained gases, and then resterilize the cans with their contents, close them again, and put them on the market. These "blowholes" may be made in either end of the can, or they may be made in the sides of the can, where they are subsequently covered with the label. Of course, it does not necessarily follow that, if a can has blown and been subsequently resterilized, its contents will prove poisonous, but it is not safe to eat the contents of such cans. Reputable manufacturers discard all blown cans.

Nearly all canned jellies sold in this country are made from apples. The apples are boiled with a preparation sold under the trade name "tartarine." This consists of either dilute hydrochloric or sulphuric acid. Samples examined by the writer have invariably been found to consist of dilute hydrochloric acid. The jelly thus formed by the action of the dilute acid upon the apple is converted into quince, pear, pineapple, or any other fruit that the manufacturer may choose by the addition of artificial flavoring. There is no reason for believing that the jellies thus prepared are harmful to health.

Canned fruits occasionally contain salicylic acid in some form. There has been considerable discussion among sanitarians as to whether or not the use of this preservative is admissible. Serious poisoning with canned fruits is very rare. However, there can be but little doubt that many minor digestive disturbances are caused by acids formed in these foods. There has been much apprehension concerning the possibility of poisoning resulting from the soluble salts of tin formed by the action of fruit acids upon the can. The writer believes that anxiety on this point is unnecessary, and he has failed to find any positive evidence of poisoning resulting from this cause.

There are two kinds of condensed milk sold in cans. These are known as condensed milk "with" and "without" sugar. In the preparation of the first-mentioned kind, a large amount of cane sugar is added to condensed milk, and this, acting as a preservative, renders the preparation and successful handling of this article of food comparatively easy. On the other hand, condensed milk to which sugar has not been added is very liable to decomposition, and great care must be used in its preparation. The writer has seen several cases of severe poisoning that have resulted from decomposed canned milk. Any of the galactotoxicons (milk poisons) may be formed in this milk. In these instances the cans were blown, both ends being convex.

One of the most important sanitary questions with which we are concerned today is that pertaining to the subject of canned meats. It is undoubtedly true that unscrupulous manufacturers are putting upon the market articles of this kind of food which no decent man knowingly would eat, and which are undoubtedly harmful to all.

The knowledge gained by investigations in chemical and bacteriological science have enabled the unscrupulous to take putrid liver and other disgusting substances and present them in such a form that the most fastidious palate would not recognize their origin. In this way the flesh from diseased animals and that which has undergone putrefactive changes may be doctored up and sold as reputable articles of diet. The writer does not believe that this practice is largely resorted to in this country, but that questionable preservatives have been used to some extent has been amply demonstrated by the testimony of the manufacturers of these articles themselves, given before the Senate committee now investigating the question of food and food adulterations. It is certainly true that most of the adulterations used in our foods are not injurious to health, but are fraudulent in a pecuniary sense; but when the flesh of diseased animals and substances which have undergone putrefactive decomposition can be doctored up and preserved by the addition of such agents as formaldehyde, it is time that the public should demand some restrictive measures.

North America, with all its strawberry short-cakes, clam-bakes, and railroad restaurants, is perhaps, after all, the land blessed with the most natural diet. Healthy food, which is the not-often-used privilege of the rich in Europe, abounds on the table of the poor farmer here. Our five or six largest cities emulate the vice-centers of the Old World, and have not learned yet to sin with grace and long impunity; but the populations of our glorious rural districts, in the valleys of New England, on the Western table-lands, and in the paradise of the Alleghanies, live more faithful to nature than any white men since the days of Cincinnatus, in the golden age of Italy, and in consequence are healthier and healthier-looking than any contemporary race, the peasantry of the Tyrol and the Swiss highlands alone excepted. There we meet our physical superiors; but our inferiority is not hopeless, and if we would just fry a little less and cook more, and substitute milk for coffee, Virginia and Vermont would soon turn out boys to match the prettiest Gemsenjäger of the Alpenland. —"Dietetic Curiosities." (Oswald, Felix L., *Pop. Sci. Mon.*, 1879, **15**, 30.)

# Some Eighteenth-Century Evolutionists*

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A SATISFACTORY history of the theory of descent is a chapter in the records of human opinion that is still to be written. Meanwhile the subject is one about which persistent errors and illusions of historical perspective prevail. The popular mind appears to be firmly possessed by the belief that the doctrine of the evolution of species was a scientific innovation first promulgated, or at all events first cogently defended, by Darwin; the fame of the natural-selection hypothesis has become so great that its author figures, in the eyes of the great public, as the parent of the whole transformist system, while the earlier half century of controversy in behalf of that doctrine, under the leadership of Lamarck and of Geoffroy St. Hilaire, is forgotten. How far even instructed persons may suffer from this illusion of perspective was illustrated in the recent commemorations of the Emerson centenary. More than one of the eulogists of the great moralist of New England descanted upon his very un-Darwinian lines which tell how

> striving to be man, the worm
> Mounts through all the spires of form,

as a remarkable "anticipation of Darwin" and an example of the power of the poetic imagination to divine scientific truth. But the lines in question, added to the editions of Emerson's *Nature* after 1849, are, of course, merely an epigrammatic versification of the main doctrine of Robert Chambers' *Vestiges of Creation,* published in 1844; and the conception they express could hardly have been a very original one at any time after the appearance of Lamarck's *Philosophie Zoologique* in 1809.The same confusion is illustrated again in the persistency with which writers on Tennyson take it for granted that the famous passage in *In Memoriam* about nature,

> so careful of the type,
> So careless of the single life,

is an echo of the *Origin of Species,* which in reality did not appear until at least fifteen years after this part of the poem was written. Even Mr. Frederic Harrison has fallen into this error; and Mr. G. K. Chesterton has recently written about Tennyson in a way calculated to give the error fresh currency. But even those who do not forget that the theory of the transmutation of species has been a familiar and influential doctrine, established upon fairly conclusive arguments ever since the beginning of the nineteenth century, are likely to forget the fact that the doctrine, in its proper modern form, takes its origin, as a respectably fathered and militant hypothesis, in France in the middle of the eighteenth century. The histories of the theory of evolution† mention, indeed, a number of names in the eighteenth and in many earlier centuries, with which vague and more or less eccentric foreshadowings of the now-accepted doctrine are connected. But the books on the subject which we have in English are unfortunately either inadequate or inaccurate or both; and they rather disguise than reveal the real character and significance of the evolutionist movement in the eighteenth century. Many of them—Mr. Clodd's book, for example, and Huxley's essay, and Professor Packard's *Lamarck,* as well as the French works of Perrier and of Quatrefages on the precursors of Darwin—ignore some of the most important and most influential eighteenth-century evolutionists; Professor Osborn's survey (*From the*

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† In this paper the word "evolution" is used in its common contemporary sense, as meaning the descent of species from earlier species. The reader will, however, remember that in the eighteenth century the same term was employed to designate the process of the generation of individual organisms as conceived by the preformationist —i.e., the process of the literal "development" or unfolding of the ready-made and preëxisting parts of the embryo. Most "evolutionists" in this eighteenth-century sense were not evolutionists in the more modern sense in which the word is here used.

*Greeks to Darwin*, 1894) is more comprehensive but regrettably inaccurate. There is therefore some occasion for a fresh attempt to clear up some points in the earlier history of the central conception of modern biology.

It is unfortunate that the eighteenth-century manifestations of evolutionism should have so generally been grouped, by those who have written of them, in one class with the ancient adumbrations of Darwinism, as if all alike were merely interesting historic accidents. The ancient foreshadowings of the doctrine were, indeed, little more than happy but fortuitous guesses of ingenious minds. But the mid-eighteenth century outcropping of the theory was a natural—one may almost say an inevitable—consequence of the progress which had up to that time been made in natural science. And the theory found expression, not in the sporadic utterances of an obscure philosopher here and there, but in the best-known writings of three of the most celebrated leaders of the opinion of their time; so that, however little it may have gained acceptance, the theory must have been pretty widely known among their contemporaries. It is, of course, a fact sufficiently familiar that Buffon in 1749 propounded the conception of the transformation of species as a possible hypothesis; that he pointed out the homological evidence in favor of such an hypothesis, and tended in some passages to accept it; but that, in his most important passage on the subject, he rejected it, partly on grounds of religious orthodoxy. Professor Packard, in his life of Lamarck, has recently presented an interesting study of Buffon's exact position in the matter. These equivocal expressions of Buffon's are, however, commonly spoken of as if they were unique in their period; whereas the same hypothesis was put forward, within a decade, by two countrymen of his who were hardly less representative than he of the scientific progress of their generation. For one of them was the president of the Royal Academy of Science of Berlin, and the other was the editor of the *Encyclopædia*.

There were two distinct lines of development in scientific investigation and theory during the first half of the eighteenth century which led up to and suggested the theory of transformation as a natural and probable hypothesis in zoology. The first of these was the active prosecution of both observation and speculation in the field of embryology; the second was the development of the new science of comparative anatomy at the hands of Daubenton. The representative of the former way of approach to evolutionism is Maupertuis. In speaking of him, I venture to improve the occasion to give some general account of his place in the history of science, since the matter is one about which little trustworthy information appears to be generally accessible. Such an account will make the significance and the grounds of his evolutionary opinions more apparent.

## Maupertuis

Although not without some reputation as the reorganizer of the Berlin Academy—for which task he was especially imported from France by Frederick the Great—and as the director of the first expedition to demonstrate the flattening of the globe at the poles by the measurement of a degree of longitude at different latitudes, Maupertuis is usually made to play a somewhat comic role in the literary history of his century, as the rival of Voltaire for the favor of Frederick and as the victim of one of Voltaire's most ferocious satires. Although Frederick took the side of Maupertuis in that famous quarrel and caused the copies of Voltaire's libel to be burned by the hangman in all the public places of Berlin, the satirist has been more successful in gaining the ear of posterity. Immensely famous and respected as a sort of scientific oracle in his own day, Maupertuis seems now to be best known through the misrepresentations of his adversary; there is even reason to fear, from internal evidence, that some learned historians of philosophy, in the little they have to say about the "Native of St. Malo"—as Voltaire always designated him—have depended more upon the *Histoire du Docteur Akakia* than upon a careful examination of Maupertuis' own writings. Yet—in spite of the touch of vanity which sometimes made him ridiculous—and the superficiality of a good deal of his knowledge, his reputation deserves in some measure to be rehabilitated. He was by no means a great scientific investigator; his work in physics and in astronomy, which he professed for his chief specialties, seems to be of decidedly questionable accuracy and value. His celebrated "law of least action," which was the original occasion of his quarrel with Koenig and Voltaire, was a generalization vaguely conceived and ill formulated, although, as Mach has pointed out, it was taken up by Euler, the friend and partisan of Maupertuis, and transformed into an important physical principle. But in any history of the general movement of scientific thought in his century Maupertuis clearly merits a place of some distinction. For he was the possessor of a wide view of the interrelation of different scientific problems; he was an ingenious and yet often a pretty shrewd and critical interpreter of the bearing and ulterior consequences of the scientific discoveries of others; and he contributed to more than one branch of science new and important conceptions, which dur-

ing the subsequent century and a half have come into great vogue and in some cases into general acceptance.

As was the fashion of his time, Maupertuis took philosophy as well as physical science for his province; and, before considering his work in the latter domain, it is worth noting that in the former also he was the proposer of several notions, now familiar enough, which were at the time relatively novel and contrary to the prevailing intellectual fashions. As a moralist, for example, Maupertuis raised a question that has been repeated with much doleful iteration by his nineteenth-century successors, but one highly paradoxical to his contemporaries: In ordinary human life, does the sum of dissatisfactions exceed the sum of pleasures? This question he answered in a pessimistic sense, in an age when a superficial optimism was the proper note among enlightened *philosophes*—and was reproached for it by the writer who, a few years later, was to produce the *Poem on the Lisbon Disaster* and *Candide*. In laying down the logical conditions for dealing with such a question, Maupertuis anticipated Bentham and the "moral arithmetic" of the Utilitarians, by elaborating a species of hedonic calculus, in which careful definitions are offered, not only of the nature of pleasure and pain, but also of the several dimensions of each that must be reckoned in assessing the relative value of any two "sums of pleasure," or of its contrary. As a political theorist, also, he shows himself a precursor of the English Utilitarian school, at a time when nearly all the new systems of political philosophy were based upon some form of the conception of "natural rights" or "natural law." In his "Eloge de M. de Montesquieu" he criticizes the political doctrine of the "Esprit des Lois," which rests, he says, upon the assumption that there inheres in human relations *"un certain rapport d'équité"* which man's reason immediately recognizes. "It is not," writes Maupertuis, "such a principle as this that should be accepted as the fundamental principle of legislation; this is too obscure, too vague, too susceptible of different interpretations; it would leave too much to the arbitrary judgment of the legislation." The only safe guide in legislation is the *principe du plus grand bonheur.* "The problem of the legislator is simply this: A multitude of men being collected together, to procure for them the greatest sum of happiness possible. It is upon his principle that all systems of legislation should be based." All this reads like so many sentences from Bentham himself, and the resemblance is by no means merely coincidental. In the ethical and political writings of Maupertuis and of Helvétius we have the headwaters of the important stream of utilitarian influence which became so broad and sweeping a current through the work of the Benthamites. Bentham read Maupertuis early—perhaps about 1770, in his twenty-second or twenty-third year, thinks a recent writer on the subject—and, although he had already got the suggestion of his doctrine from Priestley and Helvétius and Beccaria, he found, as he himself tells us, his utilitarian tendencies strengthened and corroborated by his reading of the "Essai de Philosophie Morale." The utilitarian political teaching of Maupertuis was enunciated at least three years before the publication of a similar doctrine in the book of Helvétius (*De l'Esprit,* 1758); and that book, Mr. John Morley has said, "contained the one principle capable of supplying such a system of thinking about society as would have taught the French of that time in what direction to look for reforms." The work of Beccaria, the third of the early influences upon the mind of Bentham, was still later in date of publication (1764).

In treating of the relation of scientific method to theology, Maupertuis—although professing a somewhat perfunctory religious orthodoxy—criticized the favorite eighteenth-century argument for theism—the so-called argument from design—in which the deists no less than the orthodox of the period found the principal basis of their religious philosophy; and his criticism upon it is just such as a Darwinian might now make. It closely resembles, indeed, the criticism of the same argument that Romanes put forward long afterward as a special outcome of Darwinism. Many, says Maupertuis, have found an evidence of design in the marvelous adaptation of the organs of animals to their needs. But "may we not say that, in the fortuitous combination of the productions of Nature, since only those creatures *could* survive in whose organization a certain degree of adaptation was present (*ou se trouvaient certains rapports de convenance*), there is nothing extraordinary in the fact that such adaptation is actually found in all those species which now exist? Chance, one might say, turned out a vast number of individuals; a small proportion of these were organized in such a manner that the animals' organs could satisfy their needs. A much greater number showed neither adaptation nor order; these last have all perished. . . . Thus the species which we see to-day are but a small part of all those that a blind destiny has produced." Maupertuis did not dogmatically maintain the anti-teleological position which this criticism tended to justify; he only maintained that zoology cannot assist theology, because the former has no need of teleological explanations and can sufficiently ac-

count for the degree of adaptation which exists on the principle which we should now call that of the survival of the fittest, i.e., of the best-adapted.

Maupertuis had also his own theories in metaphysics; but these are so closely connected with his evolutionary views that the two should be considered together. I turn, then, to mention his work in promoting new ideas in natural science. He was the first to introduce the Newtonian physics and astronomy into France. In the face of a good deal of opposition, he successfully disseminated the doctrine of attraction among the learned; and it was apparently from him that Voltaire acquired a sufficient smattering of physics and astronomy to enable him to write his *Eléments de la Philosophie de Newton* (1738). As became the president of an academy, Maupertuis undertook, in his *Lettres* and *Lettres sur le Progres des Sciences*‡ to sum up certain of the most important gains that had been made by scientific inquiry, and to lay down a program of experimental investigations next to be undertaken. These investigations, he urged, should be supported by the state when they are too elaborate or too expensive to be undertaken by private enterprise. Some of his suggestions are pretty fantastic and impracticable; but the greater number show good sense and a keen appreciation of the importance of systematic experimentation, even in sciences where experimental methods had as yet been little used. He recommends, among other things, the exploration of the north and south polar regions, and of the interior of Africa, for the settlement of the chief unsettled questions in geography; urges the employment of experimental methods of zoology, especially in the study of the problems of heredity; advises specialization in medical practise; proposes the utilization of the bodies of condemned criminals for experiments on the etiology of disease; calls for the prosecution of systematic experiments with electricity, and the abandonment of premature efforts to make practical use of that force, before its properties and behavior had been adequately investigated; and indicates the possibility of the prosecution of certain *expériences métaphysiques*—i.e., of investigations in what we should now call experimental psychology. He concludes his program somewhat humorously with an enumeration of *récherches à interdire*—namely, those "chimeras of science" the philosopher's stone, the quadrature of the circle, and perpetual motion. In regard to the first of these, however—the transmutation of elements—he points out that the thing cannot be shown to be inherently impossible. For there are several legitimate hypotheses about the constitution of matter which are compatible with the possibility of transmutation. It is not unlikely, for example, that "matter is composed of homogeneous parts," and that the elements which appear to possess irreducible qualitative differences, "really differ from one another only by the dissimilar form and arrangement of the homogeneous particles which compose them." In that case, we should not be entitled to declare it impossible to give "to such particles a different form and arrangement, which is all that would be necessary in order to transmute lead or wool into gold." The objection to the search for the means of transmuting elements is, therefore, not that it can be demonstrated to aim at the impossible, but only that in the existing state of science, the value of the goal—great as it would be—"is not great enough to counterbalance the scant probability of attaining it." When one reminds oneself of the hypotheses about the constitution of matter that have come into especial vogue since the discovery of the properties of radium, these observations strike one as the expression of a rather well-balanced judgment.

It was, however, in his conception of the methods and the possibilities of natural history that Maupertuis most evidently showed himself the possessor of a wider intellectual horizon than was common among the men of science of his time. Zoologists had as yet seen little occasion to attempt more than the careful description and classification of animals; but to Maupertuis a purely descriptive and classificatory science, which was unable to formulate any laws concerning the *processes* going on in that part of nature with which it dealt, was, strictly speaking, no science at all. He had little patience with naturalists whose view of their province was so narrow. "All these treatises on animals which we as yet have," he writes in the *Lettres sur le Progrès des Sciences*, "are—even the most methodical of them—no better than pictures pretty to look at; in order to make of natural history a veritable science, naturalists must apply themselves to researches which can make us acquainted, not simply with the form of this or that animal, but with the general processes

‡ *Oeuvres*, 1756, Tome II. The proposals contained in these letters were the special objects of Voltaire's ridicule. But—M. Desnoiresterres (*Voltaire et Frédéric*, ch. 8) to the contrary notwithstanding—Voltaire gains nearly all his effects either by deliberately misrepresenting Maupertuis, or by presenting as absurdities ideas which to the unprejudiced will rather seem evidences of soundness of judgment. The Kantian idealist of our time, for example, will find some lack of point in this attempt at the ironical "*Le candidat* [Maupertuis] *se trompe, quand il dit que l'étendue n'est qu'une perception de notre âme. S'il fait jamais de bonnes études, il verra que l'étendue n'est pas comme le son et les couleurs, qui n'existent que dans nos sensations, comme le sait tout écolier.*"

of nature in the production of animals and the conservation of them." The general processes which Maupertuis thought it especially important that zoological science should investigate are those through which animal individuals and species have come to have the differences of form and function that distinguish them. Maupertuis, in a word, appears to have clearly envisaged the genetic problem in biology, at a period in the history of thought when genetic problems generally were little considered. The center of interest in zoology therefore lay, for him, in the problems of embryogeny and of heredity. Although not himself an anatomist, he made himself familiar with investigations made by others on the minute anatomy of the embryo. And, as I have intimated, he never tired of insisting that the facts of heredity should be investigated, in the case of animals, by experiments in the interbreeding of species and varieties, and, in the case of human beings, by a collation of family histories.

The opinions of Maupertuis on these matters are expressed chiefly in the work called *Vénus Physique* (1745) and in the *Système de la Nature* (1751). The latter first appeared in the form of a Latin dissertation ostensibly delivered at Erlangen by one Dr. Baumann. Maupertuis found it expedient thus to shelter himself against reproach on account of any heterodox tendencies that the book might be found to contain. Four editions—all but the first in French—were called for within four years, and the author soon assumed responsibility for his work. In the *Vénus Physique* Maupertuis essayed the popular style, and the book is consequently marred by passages written in an abominably rhetorical and affected manner. But, nonetheless, it constitutes, if I am not mistaken, the first important attack made in the eighteenth century upon the theory of the preformation of the embryo. Harvey had advanced the doctrine of epigenesis nearly a century earlier, but his arguments had failed to convince his successors, and his observations upon the chick had been shown by Malpighi to be partially erroneous. At the time when Maupertuis wrote, preformationism had long been the ruling doctrine in embryology; an immense weight of scientific authority was in its favor. Among the philosophers Malebranche and Leibniz had argued for it, among the great physiologists and anatomists Swammerdam, Redi, Malpighi, Leeuwenhoek, Winslow, and Haller had taught it. Bonnet was yet to give it its most elaborate exposition and defense; and three quarters of a century later it was still to find an adherent in Cuvier. The *Vénus Physique* is a review of the preformation theory in its several forms, designed to show that the evidence against it is conclusive.

Of the arguments for epigenesis which Maupertuis offers, it is not possible, in this brief paper, to give any sufficient account. He relies in part upon the observations of Harvey—and in so doing shows himself not quite abreast of the anatomical knowledge of his time, since, correct as Harvey's main conclusions were, the observations upon which he based them had already been superseded. Chiefly, however, Maupertuis rests his case upon the facts of double heredity and hybridism. If the embryo be truly "predelineated" in the ovum or in the "zoosperm," how, he asks, can it come about that it inherits the specific or individual characters of now one and now the other parent, and often of both? The preformationists had, of course, their devices for explaining away this pretty obvious difficulty; but Maupertuis finds it easy to show that the explanations are altogether inadequate when they are compared with even the common and easily observable facts of heredity. His reasoning is especially effective when he cites his own investigation of the transmission of hexadactylism (*sexdigitisme*) through several generations of a certain German family whose records he had examined, and points out how little the preformation hypothesis could account for the transmission of such a peculiarity through male and female parents alike, its progressive disappearance as succeeding generations more and more intermarried with persons having the normal number of digits, and its occasional atavistic reappearance in remote descendants. In view of these classes of facts, he declares, the encasement theory must be abandoned, and the conclusion must be accepted that the embryo is no ready-made article, preëxistent from the creation of the world, but a new birth, the product of a true genesis—not, indeed, a genesis of life itself, but of a new and unique combination and intermingling of already living elements contributed by both parents alike. These arguments and this conclusion, it should be remembered, were advanced by Maupertuis more than a decade before the publication of the great work of Kaspar Friedrich Wolff (*Theoria Generationis,* 1759), from which the modern revival of the doctrine of epigenesis is usually dated. The conception of epigenesis held by Maupertuis was, moreover, far more complete and accurate than that which Harvey had put forward a century earlier. For, although Harvey had asserted *marem et foeminam pariter efficientes causas esse generationis,* he had denied that there can be any physical interpenetration of ovum and spermatozoon, and had declared that fecundation consists in the communication of a purely immaterial force. It was in order to make this a little more intelligible that

Harvey had worked out his famous analogy between the conception of the embryo in the uterus, and the conception of an idea in the brain.§ All this Maupertuis remarks, is an *idée étrange*; where we have double heredity we must assume a communication of corporeal elements, vehicles of that heredity, from both sides. And, as Maupertuis also observed, the supposed fact by which Harvey had justified this singular conclusion had already been rendered more than questionable by later investigations of Verheyen.

Having established these essential principles to his satisfaction, Maupertuis proceeded to formulate an hypothesis concerning the nature of the fundamental physical process presupposed by the facts of heredity, on the one hand, and of variation, on the other. The formation of an embryo must, he conceived, be due to the combination in a new organic union, of a great number of living corporeal particles, derived usually from both parents, each of which particles carries with it a sort of organic memory (*souvenir*) of the life of the organism to which it formerly belonged, and thereby tends to unite with the other particles in such a way as to produce a new organism of the same species.‖ This process of recombination of already living particles was held by Maupertuis to be governed by something more than the laws of mechanism; embryogeny was for him no mere process of juxtaposition, under the laws of gravitation, of so many inert atoms; the ultimate units of the newly constituted living being all possess their own self-contained law of development and their own distinctive selective affinities for certain other units. Nonetheless, purely mechanical displacements of parts also take place; and to these in part, he supposed, is due the occurrence of monstrous forms, and many of the more ordinary variations from the hereditary specific type. Moreover, the elementary units which, coming from the parents, combine to form the embryonic offspring, in part carry with them a similar sort of organic memory of the particular and individual characters of the parent, and so tend to develop those characters in that offspring; but in part also they are free from this tendency, and carry with them rather the traits of more remote ancestors (atavism), and some of them may even be wholly independent of hereditary predetermination. It is these especially which, in Maupertuis's hypothesis,¶ constitute the explanation of the general tendency to variation in animals, which he recognized to the full. If one must have a further explanation why the transmitted corpuscles tend to reproduce the characters of the parents, Maupertuis suggests that perhaps there enters into the embryo a separate germ from each part of the body of the parent of which the character is reproduced; in other words, he proposes a hypothesis similar to the Darwinian theory of pangenesis. All these ideas are put forward by Maupertuis only as so many likely explanations of facts which, as he insisted, needed more adequate analysis and explanation than the embryological doctrines of the time afforded; his pangenetic theory, in particular, he regarded only as *une conjecture bien hardie, mais qui ne serait peut-être pas destituée de toute vraisemblance.*

What is noteworthy in these hypotheses is the group of truths which they involve incidentally. From Maupertuis' exposition of them it is clear that he had been led, by his reflections upon the facts of heredity, to recognize (*a*) that there is a constant tendency to variation in animals by reason of their double heredity; (*b*) that there is a further tendency to spontaneous and accidental variations, due in part to mechanical displacements or chance combinations among the ultimate particles of which the embryo is composed; (*c*) that these variations —and possibly also new characters acquired during the lifetime of the parent**—tend to be perpetuated through heredity, provided that they do not unfit the animals that possess them for survival in their environment, and provided also that they are not gradually obliterated through interbreeding with animals that do not possess them. To one who thus emphasized the factors making for variation and for the conservation of variations, the theory of the mutability of species necessarily appeared more

§ Harvey's reason for this opinion lay in his failure to discover any traces of the spermatozoon in the uterus. His own words are *"Quoniam nihil sensibile in utero post coitum reperitur; et tamen necesse est ut aliquid adsit, quod foeminam foecundam reddat; atque illud, ut probabile est, corporeum esse nequeat: superest ut ad merum conceptum, specierumque sine materia receptionem, confugiamus"*—i.e., the ovum is fertilized by being impregnated with a general concept! (*De generatione animalium*, 33.)

‖ *Vénus Physique*, Pt. II., ch. 5. We must suppose *"que la liqueur séminale de chaques espèce d'animau, contient une multitude innombrable de parties propres à former par leurs assemblages des animaux de la même espèce."*

¶ *Loc. cit.* It is likewise to be assumed *"qua dans la liqueur séminale de chaque individu, les parties propres à former des traits semblables à ceux de cet individu sont celles qui d'ordinaire sont en plus grand nombre, et qui ont le plus d'affinité; quoiqu'il y en ait beaucoup d'autres pour des traits différents."*

** Maupertuis raises the question concerning the inheritance of acquired characters, but suspends judgment upon it, and calls for further experimentation. *"Ce serait assurément quelque chose qui mériterait bien l'attention des philosophes, que d'éprouver si certaines singularités artificielles des animaux ne passeraient pas, après plusieurs générations, aux animaux qui naîtraient de ceux-là."*

natural than the theory of their fixity. Maupertuis thus passes at once from his theories of heredity to propound the hypothesis that all species may have come from a single primitive pair through the gradual accumulation and transmission of divergent variations. Granting these facts about variation, he writes,

> Would it not be possible to explain by means of them how the multiplication of the most dissimilar species might be traced back to [*aurait pu s'ensuivre de*] only two individuals? Such species would have owed their origination merely to the accidental production of certain embryos [*à quelques productions fortuites*] in which the elementary parts had not retained the arrangement which they had had in the parent animals. Each degree of deviation [*erreur*] would bring about a new species; and by means of repeated departures from the original form [*à force d'écarts repétés*] there would have come about the infinite diversity of animals that we see to-day:—a diversity which may in time increase still further, but to which it may be that the lapse of centuries will bring only imperceptible additions (*Système de la Nature, XLV*).

In the *Lettres* Maupertuis again writes, apropos of the inheritance of certain congenital individual variations in the human species: "I hold that these supernumerary digits are, at their first appearance, nothing but accidental variations. . . . But these variations once well established [*confirmées*] through a sufficient number of generations in which both sexes have had them, constitute [*fondent*] species; and it is perhaps thus that all species have multiplied." In fact, for Maupertuis the difficulty lay in explaining, not how species are transformed, but why they are so stable.

It will be seen that Maupertuis puts forward his theory of transformation only as a likely hypothesis, not as a settled truth. But it is an hypothesis for which he clearly enough indicates his own preference; and it is certain, from a passage of Diderot's which I shall presently quote, that his contemporaries looked upon him as the typical representative of the doctrine of the descent of all species from a primitive type. Yet the significance and originality of the work of Maupertuis lie not so much in his explicit enunciation of the theory of descent, as in the fact that he (1) insistently called the attention of naturalists to the problems connected with the genesis and transmission of variations; (2) framed a conception of the processes involved in embryogeny and heredity which made the mutability of species seem antecendently the more natural and more probable hypothesis; (3) indicated a program for systematic observation and experimentation with reference to heredity and to the effects of interbreeding, which, if carried out, would have transformed zoology; (4) intimated that Nature produces a far greater number of types and of individuals than she can maintain, and that among all these variant types there is constantly taking place a process of natural selection whereby those unfitted to the conditions of their life are exterminated; (5) explained the adaptation of animals to their environment solely as the result of these conjoint processes of variation and selection.††

It remains to point out that the embryological doctrines of Maupertuis were intimately connected with certain metaphysical conceptions, of a type that has often since shown itself to be peculiarly congenial to minds trained in biology. The *Système de la Nature* is primarily an exposition and a defense of the theory that all matter possesses some sort of consciousness—a "monism" similar to that of which Haeckel is the contemporary prophet. A purely mechanistic materialism, such as the atomism of the Epicureans, or the crude notions which La Mettrie had recently put forward, seemed to Maupertuis an evident absurdity; "In order to overthrow such a system," he writes, "one need do no more than ask those who hold it how it would be possible for atoms without intelligence to produce an intelligence." Mere mechanism appeared as

†† Professor Osborn, unlike most of the historians of evolutionism, makes some mention of Maupertuis, but classifies and describes his doctrines in a very curious fashion. He classes the president of the Berlin Academy, as well as the editor of the *Encyclopædia*, with such "evolutionists" as de Maillet (who "derived man from *l'homme marin*, the husband of the mermaid"), and Duret (who asserted that there were trees in Scotland, the leaves of which, falling on one side into the sea became fishes, and falling on the other side on land, became birds). Of all these equally Professor Osborn says: "They were not actually in the main evolution movement; they were either out of date or upon the side-tracks of thought. They can be sharply distinguished from both the naturalists and the philosophers in the fact that their speculations advanced without the support of observation, and without the least deference to inductive canons." Such a characterization, applied to men like Maupertuis and Diderot, certainly fails somewhat in deference to the ordinary canons of historical accuracy. Professor Osborn mentions, indeed, that "an obscure article" (the "Système de la Nature") by Maupertuis "has been unearthed in the course of the present diligent search for all the prophecies of evolution," and a partially correct account is given of some of the contentions of that writing. But no clear indication is given of the grounds of the evolutionism of Maupertuis; and the writer of *From the Greeks to Darwin* appears to have been unacquainted with the *Vénus Physique* and to have ignored the work of Maupertuis in the rehabilitation of the doctrine of epigenesis. He implies also that Buffon's theory of generation appeared earlier than that of Maupertuis, which is not the case. The general conception of the "evolution movement" and of the relative importance of its several eighteenth-century representatives, in Professor Osborn's book, seems to the present writer misleading.

little capable of explaining the phenomena of organic life as it was of explaining the phenomena of consciousness; especially manifest, Maupertuis thought, is the inadequacy of purely mechanical causes to account for the processes which he conceived to be involved in the formation of the embryo. "A blind attraction uniformly distributed throughout all particles of matter can not serve to explain how these particles arrange themselves to form even the simplest of organized bodies. If they all have the same tendency, the same power, to unite with one another, why is it that certain elements go to form the eye, certain others to form the ear, etc.? Why this marvelous arrangement? Why is it that the various elements are not united pell-mell?" The combination of material particles to form a living organism seemed to imply a principle of selection, a species of elective affinity between the particles, which could not be reduced to physical or chemical categories; and the singular fact of heredity, the transmission of qualitative similarities from one organism to another through whatever minute bodies serve as the vehicles of heredity, seemed to imply the possession by those bodies of something which could only be conceived under the analogy of conscious memory. It is necessary, then, to attribute to each particle of matter the possession of some rudimentary forms of sentiency, memory and volition. (*Si l'on veut dire sur cela quelque chose qu'on conçoive, quoiqu'encore on ne le conçoive que sur quelque analogie, il faut avoir recours a quelque principe d'intelligence, a quelque chose de semblable a ce que nous appellons désir, aversion, mémoire*). Maupertuis does not forget the radical difficulty which has been urged against the identification of the *res cogitans* and the *res extensa* ever since Descartes—the difficulty, namely, that the attributes of consciousness and extension have nothing in common, and that neither can thought be conceived as extended, nor extended matter as possessing the unity characteristic of conscious thought. Maupertuis certainly cannot be said to meet this difficulty; but he evades it by a device which has been much employed since his time by metaphysicians of opinions kindred to his. The objection in question, he avows, would be a legitimate one against any doctrine that actually asserted the identity of matter and consciousness, reducing matter to thought, or thought to a form or function of matter. But if we say that thought and extension are not things, but properties—distinct but joint properties of a common subject—the difficulty, he contends, disappears. This *tertium quid*, of which thought and extension are to be defined as coexisting properties, is something "of which the essence is unknown to us." Maupertuis at this point appears, on the one hand, as repeating the dialectical strategy of Spinoza, a philosopher almost wholly ignored in the eighteenth century; and, on the other hand, as a precursor of Mr. Herbert Spencer, with his conception of the "double aspect of an ultimately unknowable substance." Maupertuis, however, was not a psychophysical parallelist; on the contrary, as I have pointed out, the sentiency which he attributed to matter was regarded by him as an essential factor in the explanation of physical events.

How Maupertuis would have reconciled the apparent—even though "transfigured"—realism of this doctrine of conscious matter with the idealistic view of the subjectivity of the perception of space, which he expresses in one of the *Lettres*, it is impossible to say. It may be that it never occurred to him that the two opinions were discrepant; it may be that he conceived it possible to reconcile them; and it may be that the idealistic view, which was published later than the other, implied the abandonment of the realism of the mind-stuff theory. As it is, we can only say that, as a metaphysician, Maupertuis has the apparently contradictory distinction of having given utterance, during the middle decade of the eighteenth century, to the favorite contentions of both the realism and the idealism of the nineteenth.‡‡

## Diderot

Diderot was in even less degree than Maupertuis a contributor to the details of scientific knowledge; and the contrast between the work of the interpreter and that of the investigator of the facts of science is well shown in the relation of his theories to the discoveries of Daubenton. It was the great associate of Buffon who laid the foundations of the science of comparative anatomy, which was to furnish the most important arguments in favor of the theory of evolution; a French writer (Vicq d'Azyr) has even gone so far as to say that "to the merit of having made a beginning of that science Daubenton has added the merit of having carried it through to completion." After the publication of the third and fourth volumes of the *Histoire Naturelle*, an important body of facts and comparisons relating to the anatomy of the vertebrates was accessible to all

‡‡ *Lettre* IV. *"Sur la manière dont nous apercevons."* Maupertuis expresses this idealistic conclusion in these terms: *"Refléchissant done sur ce qu'il n'y a aucun rapport, entre nos perceptions et les objets extérieurs, on conviendra que tous ces objets ne sont que de simples phenoménes: l'étendue, que nous avons prise pour la base de tous ces objets, pour ce qu'en concerne l'essence, l'étendue elle-même ne sera rien de plus qu'un phenoméne."*

readers; it is one of the most serious blots upon the reputation of Buffon as a man of science that he failed to appreciate the value of this body of detailed knowledge, and in a subsequent edition of the work cut out Daubenton's anatomical contributions —to the great grief and disappointment of their author. Now the publication of the main facts of comparative anatomy brought clearly to light the striking homologies that run through the structure of all the vertebrate species. Daubenton himself, however, was not the man to see that these homologies suggested, and went far to justify, the hypothesis of the descent of all such species by progressive variation from a common ancestral prototype. His talent was not for the making of hypotheses, but for the collation of facts; he was a cautious and conservative man, capable of infinitely patient and accurate observation, but apparently not capable of penetrating to the significance of the facts which he observed. Even when the evolutionary hypothesis had been put forward by others, he gave it no encouragement; and it was apparently with the purpose of combating it that he contributed a paper to the French Academy of Sciences in 1764, arguing that the anatomical differences between man and the orangoutang are radical, and that man's general structure is elaborately adapted to the maintenance of the erect attitude, as the structure of the ape is not (*Memoires de l'Académie des Sciences,* 1764, p. 568). Similarly he argued, in his introduction to the natural-history volume of the *Encyclopédie Méthodique* (1783), that man differs so essentially from the animals, even in his anatomical character, that he ought not to be classed with them; and he expressed surprise that "it has been possible for a celebrated naturalist to place man in the rank of the quadrupeds, and to associate him in the same class with the monkeys, the lemurs and the bats."

Minds of another type, however, at once saw how the facts laid bare by Daubenton demanded for their satisfactory explanation a hypothesis such as Maupertuis had already come upon from another line of inquiry. Even Buffon, as is well known, pointed out, in the "Histoire de l'Âne" in the fourth volume of the *Histoire Naturelle,* how forcibly the homologies to which his colleague had called attention suggested the idea of a family relationship between all animals.

> If [he wrote] in the immense variety of animate creatures that people the world, we choose as a starting point for our study some one animal, or even the body of man, and if we compare it with all the other organisms, we shall find that . . . there exists a certain primitive and general type [*dessein*] which can be traced out very far. . . . Even in the parts which contribute most to give variety to the external form of animals, there is a prodigious degree of resemblance, which inevitably brings to our minds the idea of an original model upon which every creature seems to have been conceived. . . . As M. Daubenton has remarked, the foot of a horse, in appearance so different from the hand of man, is nevertheless composed of the same bones, and we have at the extremities of our fingers the same small bone of horse-shoe shape which terminates the foot of that animal. [By one who considered these facts alone], not only the ass and the horse, but also man, the ape, the quadrupeds and all the animals might be regarded as constituting but a single family. If it were admitted that the ass were of the family of the horse, and differs from the horse only because it has degenerated, one could equally well say that the ape is of the family of man, that it is a degenerate man, that man and ape have a common origin; that, in fact, all the families among the plants as well as among the animals, come from a single stock; and that all animals are descended from a single animal which in the course of time, as the result of progress or of degeneration, has given rise to all the races of other animals. . . . If it were true that the ass is a degenerated variety of the horse, there would be no longer any limits to the power of Nature, and one would not be wrong in supposing that from a single being she has been able to derive all the other organized beings.

But of course Buffon finally pronounced, at least nominally, against such a supposition: *"Mais non; il est certain par la révélation que tous les animaux ont également participé à la grace de la création."* Diderot, however,—although he was himself not wholly unpractised in perfunctory and ironical professions of orthodoxy—was somewhat more outspoken; and in his *Pensées de l'Interprétation de la Nature* he declared plainly that the doctrine of the mutability of species and of their descent from a common prototype was, if not an established truth, at any rate a legitimate and a necessary working hypothesis for all future biological investigation.

The most interesting and most explicit passage on the subject in the *Pensées* has, so far as I know, been noted by no English writer; and I therefore translate it without abridgment.

> It seems [says Diderot] that nature has taken pleasure in varying the same mechanism in a thousand different ways. She never abandons any class of her creations before she has multiplied the individuals of it in as many different forms as possible. When one looks out upon the animal kingdom and notes how, among the quadrupeds, all have functions and parts—especially the internal parts—entirely similar to those of another quadruped, would not any one readily believe [*ne croirait-on pas volontiers*] that there was never but one original animal, prototype of all animals, of which Nature has merely lengthened or shortened, transformed, multiplied or obliterated, certain organs? Imagine the fingers of the hand united and the substance of the nails so abundant that, spreading out and swelling, it envelops the whole—and in place of the human hand you have the foot of a horse. When one sees how the successive metamorphoses of the envelope of the prototype—whatever it may have been—proceed by in-

sensible degrees through one kindgom of Nature after another, and people the confines of the two kingdoms (if it is permissible to speak of confines where there is no real division)—and people, I say, the confines of the two kingdoms with beings of an uncertain and ambiguous character, stripped in large part of the forms, qualities and functions of the one and invested with the forms, qualities and functions of the other—who then would not feel himself impelled to the belief that there has been but a single first being, prototype of all beings? But whether this philosophic conjecture be admitted as true with Doctor Baumann [Maupertuis], or rejected as false with M. de Buffon, it can not be denied that we must needs embrace it [*on ne niera pas qu'il faille l'embrasser*] as a hypothesis essential to the progress of experimental science, to that of a rational philosophy, to the discovery and to the explanation of the phenomena of organic life.

If the rest of the passage left any uncertainty as to the precise nature of the hypothesis that Diderot had in mind, the reference to Maupertuis and Buffon would make his meaning unmistakable.

In a later section of the book, in which he recurs to the subject, the mocking tone of Diderot's professed submission to the "teachings of the faith" only makes the more manifest the real opinion that he holds and desires to get accepted.

May it not be that, just as an individual organism in the animal or vegetable kingdom comes into being, grows, reaches maturity, perishes and disappears from view, so whole species may pass through similar stages? If the faith had not taught us that the animals came from the hands of the Creator just such as they are now, and if it were permissible to have the least uncertainty about their beginning and their end, might not the philosopher, left to his own conjectures, suspect that the animal world [*l'animalité*] has from eternity had its separate elements confusedly scattered through the mass of matter; that it finally came about that these elements united—simply because it was possible for them to unite; that the embryo thus formed has passed through an infinite number of successive organizations and developments; that it has acquired in turn movement, sensation, ideas, thought, reflection, conscience, sentiments, passions—signs, gestures, sounds, articulate speech, language—laws, sciences and arts; that millions of years have elapsed between each of these developments; that there are perhaps still new developments to take place which are as yet unknown to us; that there has been or is to be a stationary condition of things; that the being thus developed is passing out of, or will pass out of, that condition by a continual process of decline, in which his faculties will gradually leave him just as they originally came to him; and that he will finally disappear from nature forever, or rather, will continue to exist, but in a form and with faculties wholly unlike those which characterize him in this moment of time?—But religion spares us many wanderings and much labor.

Here, of course, we have not only the transformation of species, but also the sketch of a complete system of materialistic and ateleological evolutional philosophy, after the Spencerian fashion. Most of the chapters of Mr. Spencer's elaborate biography of the universe Diderot gives us in outline—its "integration of a diffused, incoherent matter," its "successive phases of physical, psychical and social development," its "equilibration" and resultant "stationary state," and finally its "alternate cycles of evolution and dissolution."

The passage first quoted, however, seems to me the more interesting of the two, not only because it is more outspoken and free from the veil of ironical piety, but also because it shows clearly the sources and grounds of Diderot's belief in the mutability of species. He had been stimulated to write largely by the recent appearance of the *Systéme da la Nature* of Maupertuis; but he ignored the embryological line of argument and rested his conclusion upon the homologies lately made known by Daubenton and dilated upon by Buffon.

Thus the decade between 1745 and 1755 was marked by the appearance of the attack of Maupertuis upon the ruling doctrine of predelineation; by the publication of the volumes of the *Histoire Naturelle,* which familiarized even the general reader with the unity of type and the homologies of structure that ran through the most diverse species in the writings of the three most celebrated French leaders of scientific opinion of the time; and by the setting forth of two distinct lines of argument in favor of that hypothesis. From this decade, then, dates the appearance of modern evolutionism, as a theory definitely formulated and based upon its proper embryological and anatomical premises.

## Herder

If certain of the French *philosophes* have received less credit than is their due for their evolutionary opinions, Herder, on the contrary, has often been praised for an early profession of faith in the doctrine of the transformation of species, whereas it is by no means clear that he did not intend explicitly to repudiate it. A German writer, Bärenbach, has written a book to show that Herder was a precursor of Darwin, and declares that in his *Ideen zur Geschichte der Menschheit* Herder laid down "the fundamental laws of the modern development theory, and of the Darwinian theory in particular," and that he gave clear expression to "the law of the evolution of organisms, and the theories of the struggle for existence and of natural selection." Professor Osborn's account of Herder's relation to the theory apparently follows Bärenbach, and as a result is rather misleading. Herder, says Osborn, probably was helped to his evolutionism by "coming under the influence of Kant's earlier views." But "Herder was less cautious than his master, and appears almost as a literal prophet of

the modern natural philosophy. In a general way he upholds the doctrine of the transformation of the lower and higher forms of life, of a continuous transformation from lower to higher types and of the law of perfectibility." "In his *Ideen,* published in Tübingen in 1806, . . . we see that Herder clearly formulated the doctrine of unity of type which prevailed among all the evolutionists of the period immediately following."

These few sentences contain a rather undue proportion of errors, and the whole exposition of Herder's position from which they are taken is substantially wrong. It is worth while, therefore, to attempt a more accurate account of Herder's attitude toward evolutionism than is to be found in the current writings on the subject. In a matter of this kind, even accuracy about dates is not wholly to be disdained; and it should be observed that the *Ideen* were published, not at Tübingen in 1806, but at Riga and Leipzig in 1784–5. Again, Herder, although once a pupil, was no disciple of Kant's; the author of the *Metakritik* would assuredly have been surprised to hear Kant called his "master;" and it is sufficiently clear, from Herder's own language, that the influence which led him to employ such expressions as have caused some to consider him an evolutionist was that of Buffon and other naturalists of the century, not that of Kant. During Herder's student days (1762–4) in Koenigsberg, indeed, it is improbable that Kant's influence could have encouraged a belief in organic, as distinguished from cosmic, evolution. On the other hand, it is not true that Herder was "less cautious" than Kant in his treatment of the doctrine of transformation; for, by the time of the *Kritik of Judgment* (1790), Kant had grasped the theory of the descent of species in all its implications and was ready to recognize it as at least a promising hypothesis; whereas Herder, in the *Ideen*, argues at length against what he calls "the improved and totally self-contradictory paradox" that animal species can depart from their divinely defined character and that man is directly related to the ape.

Yet Herder's book is certainly full of *aperçus* that come near to the evolution theory; and it unquestionably helped to produce a state of mind favorable to the acceptance of the theory. Some passages in the *Ideen,* read by themselves, might easily seem to justify the classification of Herder with the thoroughgoing evolutionists. For his position is peculiar and somewhat equivocal. Where he stands in the matter may perhaps best be shown by setting down in catalogue fashion the several contentions that he advanced in regard to the history of the animal kingdom and the relation of the lower species to man.

1. Herder clearly recognized that there had been a sequence of temporally successive forms appearing upon the globe, beginning with simpler forms and proceeding to those most highly organized; "from stones to crystals, from crystals to metals, from these to plants, from plants to animals and from animals to man, we see the form of organization ascend; and with it the powers and propensities of the creature become more various, until finally they all, so far as possible, unite in the form of man" (Bk. V, ch. 1). Each new type as it appears is dependent for its survival upon the prior existence of simpler types—dependent usually, indeed, in a very plain sense, since the newcomer commonly has the earlier-born creatures for its necessary food.

> It is manifestly contrary to Nature that she should bring all creatures into existence at the same time. The structure of the earth and the inner constitution of the creatures themselves make this impossible. Elephants and worms, lions and infusoria, do not appear in equal numbers, nor could they be created, in consistency with their natures, at one time or in equal proportions. Millions of shellfish must needs have perished before our bare rock of earth could be made a fruitful soil for a finer type of life; a world of plants is destroyed each year in order that higher beings may be nourished thereby. Even if one wholly disregards the final causes of the creation, yet even in the very raw material of Nature there lies the necessity that one being should come out of many, that in the revolving cycle of creation countless multitudes should be destroyed so that through this destruction a nobler but less numerous race might come into being (Bk. X, ch. 2). Man, therefore, if he was to possess the earth and be lord of the creation, must find his kingdom and his dwelling-place made ready; necessarily, therefore, he must have appeared later and in smaller numbers than those over whom he was to rule (*ibid.*).

Most of this Herder might have got from Buffon; and there is obviously nothing in these passages which necessarily implies the mutability of species, nothing which is inconsistent with the doctrine of special but gradual creation. Nor is there even in such a passage as this: "From air and water, from heights and depths, I see the animals coming nearer to man, and step by step approximating his form. The bird flies in the air; every deviation of its structure from that of the quadruped is explicable from its element. The fish swims in the water: its feet and hands are transformed into tails and fins," etc. (Bk. II, ch. 3). If Herder had not elsewhere seemed to deny such a theory, we might at first sight be disposed to construe this passage as an assertion of the literal transformation of species—a Lamarckian sort of transformation, due to the adaptation of organs to needs. But when the words are closely scrutinized it is evident that they

require no such interpretation. They say no more than that animals came into being in a progressive order in which the human type was steadily approximated, and in which each form was adapted to its environment.

2. In this connection, Herder liked to dwell upon the homologies of form and structure observable in all vertebrates, and indeed, as he thought, in all creatures, even those that are outwardly most dissimilar. There is a certain *Hauptform* or *Hauptplasma* in which the whole animal kingdom agrees.

> It is undeniable that, amid all the differences of the living beings on the earth, a certain uniformity of structure and, as it were, a standard form, appears to prevail, which yet is transformed into the richest diversity. The similarity of the skeletal structure of land animals is obvious; . . . the inner structure makes the thing especially evident, and many outwardly uncouth forms are in the essentials of their internal anatomy exceedingly like man. The amphibia deviate farther from this standard; birds, fishes, insects, aquatic animals—the last of which merge in the vegetal or inorganic world—deviate still farther. Beyond this our eyes can not penetrate; but these transitions render it not improbable that in marine forms, plants, and even in the so-called inanimate things the same basis of organization may rule, though infinitely more rude and confused. In the eye of the Eternal Being, who sees all things in one connected whole, it may be that the form of the ice-particle, as it is generated, and that of the snowflake that is formed upon it, may have an analogous resemblance to the formation of the embryo in the womb. We can therefore accept it as a general law that, the nearer they approach man, the more do all creatures resemble him in their essential form; and that Nature, amid the infinite variety which she loves, seems to have fashioned all the living things upon our earth after a single original model [*Hauptplasma*] of organization (Bk. II, ch. 4).

Herder had also learned from the comparative anatomists that it is a corollary to this similarity of structure that organs which function usefully in certain species appear also in other species where they have little or no apparent use or function; in other words, he knew of the existence of vestigial and rudimentary organs.

> What Nature had given to one animal as a merely accessory feature [*Nebenwerk*] she has developed into an essential feature in another; she brings it into plain view, enlarges it, and makes the other organs—though still in perfect harmony—subservient to it. Elsewhere again these subordinate parts predominate; and all organized beings appear as so many *disjecti membra poetæ*. He who would study them must study one in another; where an organ appears neglected or concealed, let him turn to some other creature in which Nature has perfected and plainly displayed it (*loc. cit.*).

3. Herder had further learned from Buffon that, within the limits of the specific type, a species may vary widely under differing climatic influences. "Those species that inhabit nearly all parts of the globe, are differently formed in almost every climate," etc. (Bk. II, ch. 3).

4. The author of the *Ideen* also recognized, and frequently dilated upon, that fact in nature which later suggested the specifically Darwinian form of the theory of descent—the fact, namely, that nature turns out more aspirants for life than she can provide with the means of living, and that there results from this situation a universal struggle for existence between species and between individuals. Herder had, in fact, been profoundly impressed by the way in which the life processes of nature seem to be the expression merely of a blind, striving *Wille zum Leben* (in the language of a later school), careless of the single life, tending only to the production of the greatest possible number of living beings, each of them competing with all the others. The discovery of this impressive and sinister aspect of nature was, certainly, the main source, alike of the most important scientific hypothesis of the nineteenth century, and of certain of the most significant and characteristic developments of nineteenth-century philosophy—especially of philosophical pessimism. The emphasis Herder lays upon this class of facts is therefore interesting and noteworthy:

> Where and when each being *could* arise, there it arose; energies [*Kräfte*] pressed in through every gate of entrance and formed themselves to life (Bk. X, ch. 2). Nature employs infinitely many germs; . . . she must needs therefore reckon upon some loss, since all things crowd one another [*alles zusammengedrängt ist*], and nothing finds room completely to develop itself (Bk. II, ch. 2). The whole creation is at war, and the most conflicting powers lie close to one another. . . . Each being strives with each, since each itself is hard-pressed for life; it must save its own skin, and guard its own existence. Why does nature act thus? Why does she thus crowd her creatures one upon another? Because she aimed to produce the greatest number and the greatest variety of living things in the least possible space; so that one subdues another, and only through the equilibrium of opposing powers is peace brought about in the creation. Every species cares for itself as if it were the only one in existence; but by its side stands another which keeps it within bounds; and it was only in this adjustment of warring species that creative nature found the means of preserving the whole (Bk. II, ch. 3).

In all this Nature (for Herder almost invariably personifies) takes no account of the individual, but rather sacrifices him ruthlessly to her "one great end, which is—not the little end of the sentient creature alone, but—the propagation and continuance of the species." And in this connection Herder anticipates Schopenhauer in picturing the pleasures of the love of the sexes, and the romantic illusions connected with that love in man, as merely a subtle trick whereby nature cajoles the individual to sacrifice himself to her larger aim. Schopen-

hauer's famous chapter on the "Metaphysics of the Love of the Sexes" is little more than an amplification of a passage in the second book of the *Ideen*. Always, Herder perceives, when the reproduction and increase of life are at stake, nature turns Machiavelian, and plays upon the egoism of the individual for her own very different ends.

> It is [he writes] particularly humiliating to man that in the sweet impulses which he terms love, and to which he attributes so much spontaneity, he obeys the laws of nature almost as blindly as a plant. . . . Two creatures sigh for each other, and know not for what they sigh; they languish to become one, which dividing nature has denied; they swim on a sea of deception. Sweetly deceived creatures, enjoy your time; yet know that ye accomplish not your own little dreams, but, pleasantly compelled, the great aim of nature. . . . As soon as she has secured the species, she suffers the individual gradually to decay. Hardly is the season of love over before the stag loses his proud antlers, the bird its song and much of its beauty, the plants their fairest colors. The butterfly sheds its wings and expires, while alone and unweakened it might have lived through half the year. This is the course of nature in the development of beings out of one another; the stream flows on, though one wave is lost in the wave that succeeds it (Bk. II, ch. 2).

But although Herder thus clearly remarked these characteristics of nature's dealings, he did not deduce from them either the biological or the philosophical consequences which have since become so familiar. It did not occur to him to find in the facts of the overproduction of organisms and the struggle for existence an explanation—such as Maupertuis had already proposed—of that progressive production and selection of more highly organized and better adapted beings, the reality of which he so fully recognized. Nor was he led, by his apprehension of a universal tendency to the maximum production of living things, to erect the notion of "unconscious will" into the central conception of a metaphysical system.

5. In his accounts of the beginnings of human society, and of the moral instincts without which society could not exist, Herder points out that these beginnings were made possible and necessary by a prior peculiarity in the physiological constitution which distinguishes the human species—namely, by the greater length of the period of helpless infancy. This, of course, is an idea upon which many evolutionary moralists and "sociologists" have latterly delighted to dwell. Herder clearly sets forth how the prolongation of infancy was the condition and the chief cause of man's moral nature—how it provided the true training school in which the featherless biped was fitted for the social state:

> The first society arose in the paternal habitation, bound together, by the ties of blood, of mutual reliance, and of love. Thus to destroy the savagery of men and to habituate them to domestic intercourse, it was necessary that the infancy in our species should continue for some years. Nature held them together by tender bonds, so that they might not separate and forget one another, like the beasts that soon reach maturity. The father becomes the teacher of his son, as the mother has been his nurse, and thus a new tie of humanity is formed. Herein lay the ground of the necessity of human society, without which it would have been impossible for a human being to grow up, and for the species to multiply. Man is thus born for society; this the affection of his parents tells him, this the years of his longer infancy show (Bk. IV, ch. 6).

This conception, however, was by no means a new idea of Herder's own. It is, therefore, a little curious to find the idea put forward by evolutionary writers at the end of the nineteenth century as a fresh and striking discovery. Even so learned a man as the late Mr. John Fiske supposed it to be a great novelty, and even conceived that he himself was the original discoverer of it. In the preface to his *Destiny of Man* Mr. Fiske writes: "The detection of the part played by the lengthening of infancy in the genesis of the human race is my own especial contribution to the doctrine of evolution, so that I feel somewhat uncertain as to how far that subject will be understood." But the thing had been detected, not merely by Herder but also by Pope, some fifty years before him; and that Mr. Fiske should have forgotten the fact only shows how general are the misapprehensions which concern the history of biological conceptions. Pope wrote, in the Third Epistle of the *Essay on Man* (1733):

> Thus beast and bird their common charge attend,
> The mothers nurse it, and the sires defend;
> The young dismissed, to wander earth or air,
> There stops the Instinct, and there ends the care.
> A longer care man's helpless kind demands,
> That longer care contracts more lasting bands . . .
> Still as one brood, and as another rose,
> These nat'ral love maintained, habitual those:
> The last, scarce ripened into perfect man,
> Saw helpless him from whom their life began;
> While pleasure, gratitude and hope combined
> Still spread the interest, and preserved the kind.

Pope got the suggestion of this from one of Bolingbroke's "Fragments;" but Bolingbroke had missed the main point, which Pope, in this case more original than his guide, clearly perceived. "If men," Bolingbroke had written, "come helpless into the world like other animals; if they require even longer than other animals to be nursed and educated by the tender instinct of their parents; it is because they have more to learn and more to do; it is because they are prepared for a more improved state and for greater happiness."

Bolingbroke failed to see that the most important

consequence of the greater length of human infancy lay in its effect, not upon the child, but upon the parent—in creating the necessity for a steady and self-sacrificing affection and for a habitual subordination of immediate and personal aims to remote and disinterested ones. It may be, then, that to Pope should be given the credit of having first called attention to the relation between the lengthening of infancy and the evolution of social morality. It can hardly be doubted that it was the passage in Pope's poem that suggested the idea to Herder.

6. In all these cases the receptive and pregnant mind of Herder had grasped and elaborated separate ideas which have since become familiar parts of the general body of evolutionary theory. But did he accept the essential doctrine of evolution itself? Did he believe in the mutability of species and in the literal descent of man from lower forms of life? If I am able to interpret his utterances correctly, he did not; on the contrary, he seems to have been at pains to express his dissent from such a doctrine—which, as we have seen, was familiar enough to the men of science at this time. There are several distinct passages in the *Ideen* in which Herder discusses the relation of man to the animal kingdom; and, in order that the reader may have the means of deciding for himself what Herder's position was, I will cite them at some length.

> There are those [he says (Bk. III, ch. 6)], who have, I will not say degraded man to the rank of a beast, but have denied to him the character of his race, and would make him out to be a degenerate animal [*ausgeartete Thier*] which in striving after a higher perfection has wholly lost the distinctive qualities [*Eigenheit*] of its species. This, however, is manifestly contrary to the truth and to the evidence of natural history; man obviously has characteristics that no animal possesses, and performs actions of which both the good and evil belong to him alone. . . . Since every animal remains true upon the whole to the character of its species, and since we alone have free will instead of necessity for our ruling power, then this difference must be investigated as a fact—for fact it undeniably is. The other questions—how man came by this distinctive characteristic; whether it was his from the beginning, or is adventitious and acquired: these are questions of a purely historical sort. Now, setting aside all metaphysics, let us confine ourselves to physiology and experience.

Herder then points out the anatomical peculiarities of man, particularly those which, as Daubenton had shown, are connected with his greatest peculiarity, the upright attitude. And in view of these considerations Herder concludes thus:

> Would the human animal, if he had been for ages in an inferior state—and if he had been formed as a quadruped in his mother's womb, with wholly different proportions—would he have left that state of his own accord and have raised himself to an erect posture? Out of the faculties of a beast, which would ever be drawing him backward, could he have made himself a man, and, even before he became a man, have discovered human speech? If man had ever been a four-footed animal, if he had been such for thousands of years, assuredly he would remain such still; and nothing but a miracle of new creation could have made him what he now is. Why, then, should we embrace unproved, nay totally self-contradictory, paradoxes, when the structure of man, the history of his species and, as it seems to me, the whole analogy of the organization of our earth, lead us to another conclusion? No creature that we know has ever departed from its original organization and adapted itself to another contrary to it; for it can operate only through the powers that inhere in its organization, and Nature is abundantly able to hold each living being fast in that state to which she has assigned it. In man everything is adapted to the form he now bears; from this everything in his history is explicable; without it nothing is capable of explanation. . . . Why should we humble in the dust the crown of our high calling, and shut our eyes to that central point in which all the radii of Nature's circle seem to converge?

In a later passage (Bk. VII, ch. 1) Herder directly discusses, only to reject, the theories of those (he probably has Monboddo especially in mind) who assert a kinship, or an identity of species, between the apes and man.

> I could wish [he writes] that the affinity of man to the ape had never been urged so far as to cause people to overlook, in seeking a graded scale [*Leiter*] of being, the actual steps and intervals without which no scale can exist. What, for example, can the rickety orang-outang explain in the figure of the Kamchatkan, the pigmy chimpanzee in the size of the Greenlander, the pongo in the Patagonian? for all these forms would have arisen from the nature of man if there had been no such thing as an ape upon the earth. . . . In point of fact, the apes and man were never one and the same species [*Gattung*]. For each race nature has done enough, to each she has given its own proper heritage. The apes she has divided into as many species and varieties as possible, and extended these as far as she could. But thou, O man, reverence thyself! Neither the pongo nor the gibbon is thy brother; the American and the Negro are. These then thou shouldst not oppress nor kill nor rob, for they are men like thee; but with the ape thou canst not enter into fraternity.

Herder expresses himself in a similar vein in a preface which he wrote for a German translation (1784) of the work in which Lord Monboddo set forth, among other things, his theory of the close relationship of man and monkey. Herder praises cordially Monboddo's taste for ancient art, and his large and philosophical way of dealing with the problem of the origin and development of human language; but he warns the reader against Monboddo's views on the orangoutang. The opinion that "*Affe und Mensch ein Geschlecht sei*," Herder marks as "an error which even the facts of anatomy contradict."

In another chapter of the *Ideen* Herder speaks of

the transitions (*Uebergänge und Ueberleitungen*) and metamorphoses (*Verwandlungen*) through which nature leads the successive orders of animals, in a fashion which seems at first sight plainly to imply the derivation of higher from lower species by ordinary descent; yet in the same paragraph he pauses to insist upon the fixity of specific types:

> It may appear that such transitions are incompatible with the definiteness of form to which every species remains true, and in which not the smallest bone undergoes alteration. But the reason for this invariability is apparent; since every creature can receive its organization only from other creatures of its own species. Our orderly Mother Nature has thus plainly predetermined the way by which any organic power should come into existence [*Wirksamkeit*]; and thus nothing can escape from its once-determined form (Bk. V, ch. 3).

It is hard to see how any reader of Herder's generation could have understood these utterances, when taken all together, in any other sense than as an assertion of the essential immutability of species and a denial of man's descent from simian or any other animal ancestors. It would appear then, that Herder never fully recognized that—as Kant put it—"the similarity of form in animals (such that they seem to be made after a common prototype) confirms the supposition that they have an actual blood-relationship, through descent from a common parent." How, in the absence of such an hypothesis, Herder would have explained the gradual appearance of progressively higher forms is, undeniably, somewhat incomprehensible. But the truth is that his whole treatment of the subject is poetical, vague, and not very careful of consistency, rather than explicit, definite, and scientific. The theory of descent was, at the time he wrote, almost a commonplace of current biological discussion; but his attitude toward it was certainly ambiguous, and apparently hostile. The author of the *Ideen zur Geschichte der Menschheit* may almost be called the father of the modern philosophy of history; but he cannot unqualifiedly be called a pioneer of modern evolutionist biology.

## Monboddo

The author of the work that called forth Herder's mingled admiration and criticism, James Burnett, Lord Monboddo, has been a good deal neglected by the historians of evolutionism; and, in spite of the fact that he was perhaps the first to make widely familiar to the British public the doctrine that man is descended from apelike ancestors, it is doubtless true that his cannot be considered a very serious contribution to the progress of zoological knowledge. This learned, original, and whimsical judge is a highly picturesque and interesting figure in the literary history of Scotland in the eighteenth century. He was one of the conspicuous leaders of the intellectual society of Edinburgh at a time when the Scotch Athens—even while Jacobite uprisings still threatened—was one of the most notable seats of scientific and philosophical inquiry in Europe. In the circle of Monboddo's intimates were such men as David Hume; Adam Smith; Hutton, the founder of modern geology; Black, originator of pneumatic chemistry and of the theory of latent heat; Robertson, the historian; Lord Kames; Dugald Stewart; and, among the men of letters, Home —a dramatist whom Monboddo preferred to Shakespeare—and Fergusson. Lord Monboddo played the host to Burns in Edinburgh, and to Johnson at his ancestral countryseat—the conversation of the two great men on this latter occasion being recorded with great gusto by Boswell, who dearly loved to see the sparks fly at the contact of opposing minds. In order to promote intellectual conviviality among his learned fellow-townsmen, Monboddo introduced the innovation of late dinners—for his literary suppers, we are told by one who attended them, "had the variety and abundance of a principal meal," and were akin to the symposia of the ancients. In this society, so distinguished for scientific attainments and for original theories in natural science and philosophy, Monboddo had the reputation of being one of the most learned and most original. His speculations about the origin of language were only less notable, as a piece of pioneering in a new science, than was the work of Smith, of Hutton, and of Black; and he had the insight to suggest—though only in a private letter—the theory of the common descent of the European tongues and Sanscrit, a language then newly made known to the Occident by his correspondent Sir William Jones. But it was felt by most of Monboddo's British contemporaries that he pushed originality in theorizing to the point of fantastic absurdity when he declared that civilized man is akin to the orangoutang and a descendant of progenitors that lacked speech and possibly had tails. The Judge's chapters on the orangoutang sent learned Britain into inextinguishable laughter, and many were the poor witticisms made at his expense. The most vigorous and most amusing of all his critics was the great representative of a common-sense conservatism, Dr. Johnson. Gibes and invectives directed against the author of so ludicrous and so scandalous a doctrine are constantly recurring in the pages of Boswell: "Sir, it is as possible that the orang-outang does not speak, as that he speaks. However, I shall not contest the point; I should have thought it impossible to find

a Monboddo; yet he exists." "It is a pity," said Johnson again, "to see Lord Monboddo publish such notions as he has done; a man of sense and of so much elegant learning. There would be nothing in a fool doing it; we should only laugh; but when a wise man does it, we are sorry. Other people have strange notions, but they conceal them; if they have tails, they hide them; but Monboddo is as jealous of his tail as a squirrel." But Johnson's final objection is expressed in these words: "Sir, it is all conjecture about a thing useless even if it were known to be true. . . . Conjecture as to things useful is good; but conjecture as to what it would be useless to know, such as whether man went on all four, is very idle." The intellectual history of the century that followed constitutes an ironical commentary on this dictum of the great eighteenth-century conservative.

Monboddo's opinions concerning the descent of man are expressed most at length in the first volume of the *Origin and Progress of Language,* published in 1773; some further hints of them may be found here and there in the letters of Monboddo recently collected and edited by Professor Knight. Those opinions are entirely incidental to his theory of language. Monboddo nowhere discusses the general biological question of the transformation of species, and possibly did not believe in the transformist doctrine as such—so that it is perhaps too much to call him, with Professor Knight, "a virtual evolutionist, holding an honoured place between Lucretius and Darwin." The main contention of his book concerns the evolution of man's language, not of man himself, and is to the effect that language can have arisen only after man had for some time lived in the political state, "which state is not natural to man any more than the language to which it gave birth." In order to establish such a theory it is desirable, if not essential, to point to instances of societies of men living without language; and it is at this juncture that Monboddo meets the difficulty by bringing forward his doctrine about the orangoutang.

That doctrine is that man and the orangoutang are one and the same species. What the orang-outang is our ancestors were. The orangoutang and chimpanzee are varieties of men that have failed to acquire the art of speech; or—what comes to the same thing, for Monboddo—our ancestors were a community of orangoutangs who succeeded in acquiring that art. It is evident that in such a contention a belief in transformation is not necessarily implied; in fact, in order to establish our descent from the orang, Monboddo seems to think it necessary to establish strict identity of species—thus implying that species cannot descend from other species. In the *Origin and Progress* he explicitly declines to generalize his doctrine. "Though I hold the orang-outang to be of our species, it must not be supposed that I think the monkey or ape, with or without a tail, participates of our nature; on the contrary, I maintain that however much his form may resemble ours, he is, as Linnæus says of the Troglodyte, *nec nostri generis nec sanguinis.*" In one of the letters in Professor Knight's volume, however, Monboddo writes: "I think the simian race is of kin to us, though not so nearly related [as the orang-outang]. For the large monkeys and baboons appear to me to stand in the same relation to us that the ass does to the horse, or our gold-finch to the canary-bird." What he conceived that relation to be, he does not tell us; but it may fairly be supposed that he was thinking of collateral descent from a common ancestral species.

Monboddo's statements of his position, and his arguments for it, are made somewhat ambiguous by the fact that he, like Herder, is not altogether clear as to what constitutes identity of species, though he notes Buffon's definition, which makes the only test of difference of species in animals to be the infertility of their offspring. Monboddo thinks we can find other criteria that are as conclusive and easier to apply. We are entitled, he holds, to assign to the same species all animals which possess in common a large—but an undefined—number of similar characteristics, provided that these characteristics appear to be essential and "such as have great influence upon their nature." Now the orangoutang greatly resembles man in his external form, his anatomical structure, and even in his "inward principle," the "natural habits and dispositions of the mind." Upon this last point of resemblance Monboddo particularly likes to dilate; it seems to be his principal criterion of unity of species. The reason why the baboons and other monkeys are, in the published treatise, denied kinship with us is that, similar to us in other respects, they lack this intellectual resemblance. Of the intellectual parts and the charm of temperament of our brother the orang, Lord Monboddo exhibits an extremely exalted opinion:

> The orang-outang has the human intelligence, as much as can be expected in an animal living without civility or arts: he has a disposition of mind mild, docile and humane: he has the sentiments and affections peculiar to our species, such as the sense of modesty, of honor and of justice; and likewise an attachment and friendship to one individual so strong in some instances that one friend will not survive the other: they live in society and have some arts of life; for they build huts and use an artificial weapon for attack and defence, viz., a stick; which

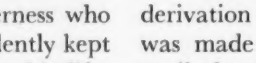
no animal merely brute is known to do. They show also counsel and design, by carrying off creatures of our species for certain purposes, and keeping them for years together without doing them any harm; which no brute creature was ever known to do. They appear likewise to have some kind of civility among them, and to practice certain rites, such as that of burying the dead.

The female orangoutang, it appears upon the testimony of Bontius the Batavian physician, is modest to the point of prudery; and some of the species are of so fine a sensibility that they shed tears copiously upon being parted from persons to whom they have become attached.

Monboddo's arguments for his theory come somewhere nearer to the proper homological proofs of evolution when he points out that the *os coccygis* is plainly nothing but a rudimentary and abbreviated tail, and that the civilized man thus carries about upon him a telltale member which hopelessly betrays the secret of his ancestry. Monboddo seems to opine that the loss of the tail by mankind has been comparatively recent, and that it is by no means universal; in justification of this opinion he adduces a number of travelers' stories that are more diverting than plausible. There is, for example, the story told by a Swedish sailor—whose credibility was vouched for by Linnæus—who saw on one of the Nicobar Islands "a race of men that trafficked and used the art of navigation, who had tails like those of cats, and which they moved in the same manner." This narration is perhaps exaggerated, Monboddo admits; but "that there are men with tails is a fact so well attested that it can not be doubted." For, setting aside all travelers' reports, and the testimony given by the ancients to the existence of races of *homines caudati,* he himself had known of a Scotch schoolmaster in Inverness who had a tail half a foot long. The man prudently kept his unusual endowment concealed during his lifetime, but it was discovered after his death—of all of which Lord Monboddo offers to bring legal evidence. The superficial and the dogmatists, he adds, will no doubt think these stories very ridiculous, "but the philosopher, who is more disposed to inquire than to laugh and deride, will not reject it at once as a thing incredible that there should be such a variety in our species, as well as in the simian tribe which is so near akin to us."

All these arguments a posteriori are really irrelevant to Monboddo's main thesis about the relation of man to the orangoutang; since those particular "simian tribes" with which alone he declares man to be akin (i.e., *satyrus* and *troglodytes niger*) are destitute of tails, and have an even more rudimentary coccyx than man. The fact that men had tails would, from his own standpoint, rather tend to show that man and orang belonged to different species than that they belonged to the same. Of this Monboddo seems to be not unaware, for he introduces his stories of tailed men as a sort of digression, and not as a part of his principal argument. As it stands, his discussion about tails seems rather to resemble the caudal vertebrae with which it is concerned—it suggests a good deal, but is not designed to bear any of the weight of proof, and is a relatively functionless appendage to the main body of his theory.

The comparative crudity and superficiality of Monboddo's speculations about the descent of man are one indication of the fact that, down to the end of the eighteenth century, the country of Darwin had made far less progress in this part of biology than had France and Germany. For Monboddo's book seems to be the nearest approach to an assertion of the mutability of species and the derivation of man from animal ancestors which was made by any generally read English writer, until the *Zoönomia* of Dr. Erasmus Darwin appeared in 1794.



Dr. Carmichael, whose article "Motives for the Cultivation of Mathematics" appears in the following pages, retired as dean of the Graduate College, University of Illinois, on September 1, 1947, and is now living in Griggsville, Illinois. His book-length poem, *What Is Man?* was scheduled for publication by the University of Illinois Press on September 1. (Because of lack of space, we were obliged to omit the long poem with which his article ended.)

# Motives for the Cultivation of Mathematics*

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THE fundamental motives for its cultivation mathematics shares with the other sciences; for they and it are equally creations of the mind and derive their characteristic qualities from the mind, subject to varying color due to the differing materials. Motives which are special to the science of mathematics, if they exist, are less fundamental and are personal rather than general in their nature. In the light of the subject matter of mathematics the basic motives may exhibit a different color or a different mutual relation from that to be observed in the light of another particular discipline; and this variation may be worthy of special consideration. In a particular age or country the general motives may operate in unbalanced proportion so that an analysis of the situation may reveal improper tendencies which demand correction. It is for the purpose of ascertaining what changes, if any, are desirable in the distribution of motive as it now operates in the cultivation of mathematics in America that the considerations of this essay are presented.

It may be possible to classify motives into logically distinct groups so that there are no omissions and no overlappings; but it serves our purpose better to divide them into classes not mutually exclusive, classes among which there are vital connections analogous to those among the various parts of a living organism. It appears also that this is the better way to exhibit the science in its true aspect as a thing of life, growing under the action of ever-varying forces and impulses.

Probably the most fundamental impulse for the advancement of knowledge is that which grows out of the pursuit of truth for truth's sake. We are fundamentally so constituted that we delight in knowing for the sake of knowing. It is hard to describe this motive, or even to conceive it, in other than a vague way. It is our most abstract and our most general motive. It actuates most powerfully our choicest spirits, moving them sometimes with a fervor akin to that of religion. A marvelous curiosity to know creates a longing that can be satisfied only by knowledge. It projects itself into the unknown and leads the researcher in ways yet untrodden to a goal which cannot be foreseen. At the outer boundary line of knowledge, faint glimmerings may be detected in the darkness of ignorance beyond. What beckons us forth we do not know. Whether it can bring us any good we have no means of foretelling. It may lead us to a tragic something which will make it necessary for us, in much pain, to cast away some of our most cherished prejudices. But, whatever lies beyond in that which is concealed from our present vision,

> We work with this assurance clear,
> To cover up a truth for fear
> Can never be the wisest way;
> By every power of thoughtful mind
> We strive a proper means to find
> To bring it to the light of day.

Delight in the beauty of truth is a central incentive to its study and creation; and this operates with a unique and peculiar power in the cultivation of mathematics. In some respects its beauties are peculiar to itself and require the trained mind to perceive them, just as the deeper beauties of music (for instance) are perceived only by the practised ear of the musician. A much larger proportion of the excellencies of mathematical truth can be enjoyed by cultivated people in general than is usually supposed; but we still lack the exposition suitable to make this manifest. Both for the individual mathematician and for mankind at large a second fundamental motive for the cultivation of mathematics is that which grows out of the pursuit of high aesthetic interests.

The leading characteristic of man is the power to think. There is nothing of higher aesthetic interest than to determine whether we can think con-

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sistently. This fundamental question can be answered in the affirmative only by exhibiting the result of consistent thinking. The existence of mathematics gives the spirit of man leave to believe in itself, since here admittedly is a body of consistent thought maintaining itself for generations, and even for millenniums.

But man is not all spirit. We cannot live by intellectual delight alone. We have to get around in a world which has trees and stones and mountains and rivers, and shifting currents of force, and even living things which dispute with us the possession of the earth or are used by us for food or beasts of burden. Many of these opposing forces are physically far stronger than we. If we are to control them it must be through a superior knowledge of them and of our common surroundings. This knowledge is necessary to our welfare. Thus as a third fundamental motive for the cultivation of mathematics we have that which grows out of the pursuit of means for interpreting and understanding our environment.

It is obvious that a single piece of mathematical work may be undertaken from considerations arising at once from two or from all of these three fundamental motives. There may be involved simultaneously the pursuit of means for interpreting and understanding our environment, the pursuit of high aesthetic interests, and the pursuit of truth for truth's sake. But it seems that there is no other motive of fundamental importance which is not included in these, either separately or conjointly. What finds its place here least naturally, perhaps, is the play motive, which has certainly operated with considerable force among a few persons who have cultivated mathematics in the spirit of amateurs. But so far as a thing of this sort has been a fundamental motive in the general development of mathematics, it may be associated with the delight arising from aesthetic consideration or from the desire to know merely for the sake of knowing.

Even such an ideal motive as that arising out of the pursuit of truth for truth's sake has in it elements of danger, owing partly to the lack of clear definition of what is involved in it. It is easy to give it glibly as one's fundamental motive and so conceal from oneself a lack of thought on the matter or the lack of deep reality or genuine sincerity in one's motives. On account of its vagueness it may give rise to a sort of mysticism which does not consort well with scientific ideals. It runs the risk of becoming a fetish, an object of excessive devotion, and of drawing the veil over necessary distinctions in the values of truths. A catalogue of all the truths in the universe would probably be useless to limited beings like ourselves. The totality is so vast that we cannot comprehend it or get about among its parts. We need some means of ascertaining what truths signify something for us, as we should otherwise be lost in the maze of all that is true and be unable to extract what is of value to us. In the pursuit of truth merely for truth's sake there is danger of giving attention alike to what properly concerns us and to what is without distinct relation to any of our needs. We require other motives to operate in the way of helping to direct our activities.

The demand for simplicity and elegance in the pursuit of aesthetic interests in science may tend to render labor effeminate. Mathematicians must attack difficult problems with the zest of red-blooded vitality. They must not be repelled by complexities and inelegancies. The problem which is confronted in the search for truth must be solved, even though it be by tedious means and with results hard to understand. To be sure, "a thing of beauty is a joy forever;" but nothing is a thing of beauty which is merely so. That beauty is not permanent which is its own entire excuse for being. In scientific truth there is a need of sincerity and high purpose back of the creation of anything of beauty. To pursue truth merely and solely for the sake of aesthetic delight tends to induce in one an admiration for the tinsels of knowledge and a joy in its more superficial elements. The greatest delight in the beauty of truth flows from its unfolding as an incident to the creation of values of profound import to mankind.

Gross utilitarianism is the obvious danger that arises from the pursuit of means for interpreting and understanding our environment. If we start out to create truth for the sake of its applications, we take a one-sided and narrow view of it. By circumscribing our vision we fail to see the connection of related parts, and our progress is soon brought to an end. Nature does not yield her secrets to him who seeks them to supply his immediate grosser needs; she rewards only a more idealistic purpose.

Worse than any of these dangers incident to an improper emphasis on the general motives is that arising in the case of an individual or a nation from false or selfish motives. From the ore of thought important truth can usually be extracted only under the heat of a glowing zeal, when the mind is surcharged with that determination which arises from strongly motivated activity. Work which proceeds not at white heat is coldly done and possesses not the fire of vitality. The mind can be brought into this fit attitude and activity only by means which are in accordance with its fundamental ways of

working. A motive growing out of the desire for selfish values inspires no such state. Only minor results may be achieved under such a spur to activity. The young researcher who looks forward to a career of useful discovery should regard the cultivation of high motives in his own mind as one of his primary and most important duties. If he is not already moved by the higher considerations, there is little hope for him; if these now operate powerfully in his thought, let him seek means to develop them more fully, let him meditate upon the things of higher importance and more profound value so that these shall ever renew and build up in him ideals of the nobler sort. One cannot successfully woo the science of mathematics (or any other science) except under the inspiration of high motives. She refuses to consort with sordid aims. She can be happy only with him of high ideals who cherishes her nobler qualities; and only to him will she yield her increase for the blessing of mankind.

Over against the dangers arising from an unbalanced emphasis on the fundamental motives are the peculiar advantages due to those activities which are inspired primarily by each of them separately. In pursuing truth for truth's sake we solve hard questions by the best methods we may; but we solve them, or else we keep them before us as an ever-present incentive to the creation of new methods of conquest and power. Difficulty never turns us aside, except temporarily while we seek new means of progress or investigate adjacent fields. The absence of apparent aesthetic satisfactions is no bar. If we cannot find the truth which delights us with its elegance and beauty, we will ascertain the best possible. We shall brood over its incompleteness until we find a way of bringing it to perfection. If the matter connects with things which appear to us vital we shall pursue it to the end regardless of practical utility of any sort. An inner spring, a necessity of our being, impels us in this direction. We are fortified in the desire to follow up our inclination here by the observation that our predecessors in laboring under a like impulse have often found results necessary to the realization of other desired ends. The unknown is too mysterious to be charted in advance. For the best means of penetrating it we must trust largely to our blind instincts, modified perhaps by past experience but still maintaining their central characteristics. In this way we not only acquire new truth, but we also develop new methods of discovery, the most elusive thing in scientific progress. A method discovered in one field under the fire of a blazing zeal enables us to surmount elsewhere other difficulties of more immediate concern, perhaps, but lacking an element which brings us in the consideration of them to the highest state of concentration and creative activity.

In pursuing aesthetic satisfactions we create a beautiful theory for the sake of our delight in it, as in the case of the theory of numbers or of abstract groups. Working in such fields with the simpler elements of mathematical thought, we make progress of a sort not at first possible with the more complex materials. We bring the theory to a higher state of perfection; there are fewer lacunae; the connections of the various parts are exhibited with clarity; we have a sense of having seen to the root of the matter and having understood it in its basic characteristics. The theory thus developed becomes an ideal in the light of which we get a new conception of what should be attained in other fields where the labor and the difficulty are greater. Results in one field of mathematics may thus become of great value in a totally different range of mathematical ideas or even in other disciplines altogether. Moreover, when such progress is attained, we often find that the tools employed in bringing it about are sufficient for dealing with more difficult matters, so that the one completed theory furnishes us not only the ideal, but also the means for further valuable progress.

A characteristic delight in mathematical truth is that which arises from economy of thought realized through the creation of general theories. When we develop the consequences of a set of broad hypotheses, we find that our results, which are attained by a single effort, have applications at once in many directions. Thus we see the common elements of diverse matters and are able to contemplate them as parts of a single general theory pleasing for its elegance and comprehensiveness.

Whether we like it or not, the evolution of humanity is a part of the cosmic process. Investigation shows that it has been so in the past. All the means by which we explain development point to the conclusion that it will remain so in the future. Since we are a part of the cosmic process, our greatest good comes with our best understanding of it. Our direct and unaided intuition does not lead us far toward comprehending the complexity of our environment. We have not the power to see directly into the explanation of things or even to devise representations of phenomena. We must seek means to assist our weakness in overcoming the difficulties of understanding. We can afford to omit none which yields, or which promises to yield, useful assistance.

Mathematics has shown itself a valuable tool in the interpretation of phenomena. It has been suc-

cessful to a marked degree. It is marvelous what sorts of things come within its scope and what connections it exhibits among them, as, for instance, in celestial mechanics, rational mechanics, kinetic theory of matter, and the theory of electricity and magnetism, to mention only a few. Through the help of mathematics we gain an increased control over nature, to our comfort and perhaps to our happiness. Through this we are able to supply our bodily wants more readily and therefore have greater freedom for meditation on the deeper things of existence—those things which we have not yet been able to bring under the domain of exact science, however vital they are to our general and to our individual development.

This contact with nature gives mathematics itself a fresh strength and a changed direction. Excursions into the domain of applications enrich the science with new conceptions, new problems, and new methods. This is borne out by the history of the past, both the remote and the more recent. It is especially noticeable in the creation of some of the most important disciplines and in the activity of some of the most renowned mathematicians.

It is instructive to consider the distribution of motive in the work of certain of the greatest mathematicians. We select a few from different ages and fields of mathematical activity and of varying temperament. The list, of course, might be greatly extended; in fact, this is done to some extent in our later consideration of certain specific topics. Those here chosen may perhaps be taken as representative of the class of mathematicians of leading importance.

We begin with Euclid. To what extent he was an original investigator is unknown; but he must have made important contributions, since otherwise his *Elements* would not so quickly have supplanted the work of his predecessors. He gave his writings a good form from the point of view of logical connection and also of pedagogical excellence. It is clear that he took delight in the beauty both of the content and of the form of his work and that he developed it primarily from the love of truth for its own sake. But his geometric postulates are what he believed to be the obvious properties of space, either of experience or of contemplation; and this part of his work may therefore be looked upon as a contribution to the study of that space in which all phenomena have their being. Still it is certain that our three general classes of motives did not operate in balanced proportion in actuating the work of Euclid, at least if we agree that the normal situation is that in which they should receive approximately equal emphasis.

Archimedes was probably the greatest mathematician of antiquity. He was inspired primarily by the love of pure science, rejoicing in the truth because it is the truth and feeling a certain contempt for the applications of truth in the way of supplying our grosser needs; and yet he was a great practical inventor and had a wide range of knowledge of phenomena, and frequently gained new strength by his contact with nature. He founded the theory of hydrostatics and contributed effectively to the initial development of mechanics and of astronomy, so that he is to be reckoned as an important figure in the history of applied mathematics even though it is true that his leading title to fame comes of his work in pure mathematics. In him we have another instance of contributions of high value associated with an unbalanced emphasis on the fundamental motives.

Fermat seems to have cultivated certain parts of mathematics for the pure love of their beauty. Probably he was hardly conscious of motive at all, since his activity was so nearly spontaneous. He made important contributions and inspired great advances, primarily in the way of new impulses to certain isolated studies.

Newton was undoubtedly moved primarily by a desire to understand and interpret natural phenomena. To this motive, therefore, we owe his invention of the differential and integral calculus (shared with Leibniz), the founding of celestial mechanics and rational mechanics in general, and the consequent development of applied mathematics in many fields of science.

A very few individual men have stood out among their contemporaries as admittedly the greatest mathematicians of their respective ages and have had the good fortune to have this verdict sustained by later generations. Euler was one of these. His prodigious activity and the penetrating character of his ideas have been alike the admiration and the inspiration of his contemporaries and successors. He has touched almost every department of mathematical science, and most modern subjects in mathematics (both pure and applied) are affected by one or more streams of influence from his genius. He was actuated by all three of our general motives. From his memoirs one may select typical cases of work actuated primarily by any one of our three classes of motives or any combination of them. Taken as a whole his work holds a just and balanced proportion among them. No man before him seems to have given so nearly equal emphasis to each of the three fundamental motives, and no one has ever maintained a more vital and vigorous enthusiasm enduring over

PIERRE DE FERMAT (1608–1665)

LEONARD EULER (1703–1783)

GOTTFRIED WILHELM LEIBNIZ (1646–1716)

JULES HENRI POINCARÉ (1854–1912)

Portraits reproduced from "Portraits of Eminent Mathematicians," by David Eugene Smith (New York: Scripta Mathematica, 1936 and 1938).

so long a career of investigation. It is significant that this balance of emphasis was coupled with discoveries of the greatest range and magnitude and influence and importance.

The case of Gauss affords another instance where the three motives worked in proper proportion, and also another instance of one holding a place of preëminent importance and influence. He was inspired by the beauty of pure truth as exhibited, for instance, in the theory of numbers; he sought a deep and penetrating understanding of things for its own sake, as in his meditations on non-Euclidean geometry; he devoted much attention to the interpretation of natural phenomena, as in his study of electricity and magnetism. The range of his influence on the further development of mathematics has been as great as the variety of motive inspiring his work. He, as well as Euler, teaches us the value of balanced emphasis in motives, at least for those who are prime movers in the development of modern mathematical science.

Poincaré is the latest example of one to stand out definitely and admittedly as the greatest mathematician of his time. He was actuated by all three motives in balanced proportion. He made fundamental contributions in many fields both of pure and of applied mathematics. No one can look at his work without seeing how he rejoiced in the beauty of truth. He has left on record a statement of his profound delight in science as the means of seeing, of knowing, and he has emphasized the fact that after all it is knowledge and insight alone which count. In the introduction to his first note on Fuchsian functions he says: "The aim which I propose . . . is to ascertain whether there exist analytic functions analogous to elliptic functions and suitable for the integration of linear differential equations with algebraic coefficients." It is known that his interest in differential equations was largely affected by their use in applied mathematics, so much so, in fact, that he was depressed when certain recent physcial theories seemed to imply that differential equations are not so fundamental to the understanding of phenomena as he had supposed. Moved by the most profound motives and of the widest variety, operating over an extremely wide range of material employing ideas of the most penetrating character, and applying his results in many directions, Poincaré stands out as the leading creator of mathematical truth in the past half century and one with few equals in the history of mankind.

Poincaré exhibited also a tendency, more marked in him perhaps than in any other mathematician, to consider a range of ideas which should probably receive increased attention owing to the growing complexity of modern mathematics (and science in general); namely, the tendency to analyze the elements of our progress in the light of broad philosophical principles. What we believe concerning the nature, the meaning, and the value of the truth with which we are concerned has a profound effect upon the operation in us of the motives for its creation.

If the illustrative cases which we have adduced are to be taken as typical of the best work in mathematics—and we have tried to make them so—they would seem to teach, among other things, that with the growth of mathematics there is a growing necessity for a proper distribution of motive in the work of the individual thinker if it is to maintain a place of preëminent importance in the development of the science. In the case of a nation or a people the same thing appears to be true in general. Early in its history a science may develop in parts in a one-sided and unproportioned way. But when it attains to maturity, and each new advance must rest on a large body of results previously derived, it is of increasing importance that a balanced distribution of motive be maintained.

It is instructive also to examine the distribution of motive in some of the most important subjects of recent progress, following the development without reference to the individuals by whom it has been brought about. To me it appears that the greatest recent advances have been made in the domain of analysis—in territory either directly belonging to it or closely connected with it by association—and that in this field are likely to arise our most fruitful investigations in the near future. Here we have a wealth of outstanding problems of broad character and of far-reaching importance. If it is so in algebra or in geometry, I am not aware of what these problems are. For this reason I have chosen the following illustrative topics primarily from the field of analysis.

Nothing is more characteristic of the modern element of rigor in mathematics than the theory of point sets. Viewed in its relatively completed state, it seems to be well removed from all considerations pertaining to an interpretation or understanding of our environment. In some aspects it seems almost to be merely a set of logical exercises created for themselves. In every way it has the appearance of a body of truth dveloped for its own sake under the impulsion of a desire to see the inner meanings and beauties of things where the intuition is in a large measure helpless. But a study of the chain of causes which led to the development of this theory carries us back to Fourier's investigations in the

distribution and flow of heat and the fundamental function-theoretic questions which were brought into prominence in the discussion of his work. Thus we find all three classes of motives operative here, though one of them appears in a concealed form, which is brought to light only by an examination of the history of the subject.

The extraordinary activity manifested a few years ago in the rapid development of the theory of integral equations was brought about by the conjunction of all our classes of motives. The theory is elegant, the body of truth developed is pleasing in its character and in its relative independence as a unit together with the many connections between it and other disciplines, and it has numerous direct applications to the interpretation of physical phenomena.

Many influences have operated to compel mathematicians to enter upon a study of functions involving an infinite number of variables. Our most simple means of representing general classes of functions of a single variable bring us to consider at once an infinitude of elements. The power series representation of a function, for instance, exhibits it as depending upon the infinite number of coefficients in such a representation; and it is natural to consider how its properties depend upon and vary with those of these coefficients. A like problem arises in connection with the Fourier expansion and with many others. When one is embarked upon the study of functions of an infinite number of variables he cannot avoid the extension of his geometric conceptions so as to involve an infinite number of dimensions. The two things go together and afford mutual illumination. It is pleasing to see properties first developed for the finite case carried over to the infinite case, and our conception of the beauty of the system of truth is greatly enhanced when we see it in all the reach of its validity, holding at once for all finite cases and for the infinite case. Naturally there are properties which distinguish and separate the finite from the infinite, so that some of them may be thought of as characteristic of the one and some of the other. We understand each of them better by seeing their analogies and differences, and thus we penetrate into a more satisfying realization of the nature and significance of the truth developed.

It is easy for one who has not meditated upon this matter to suppose that these are merely intellectual exercises with no other value than what is incident to the intellectual delight in them. But this is far from the truth. In fact, the phenomena of our environment have pressed these things upon us for a long time. Physical considerations brought us against the problem of an infinite number of variables long before we had any mathematical methods suitable for dealing with it. Let us take the intuitionally simple case of the motion of a uniform flexible string of given length and weight. This can be specified only by an infinitude of variable quantities, or coordinates, each depending on the time. In fact, physical phenomena usually depend in this way on an infinite number of variables. Lagrange's generalized coordinates furnish us one of the best and most remarkable means of studying motion—and it is to this that we try to reduce our interpretation of all physical phenomena. The Lagrange coordinates are finite in number for the simpler cases, but are infinite in number in the more general situations of nature.

For the development of the theory of functions of an infinite number of variables, both in the past and in the future, we have therefore the strongest sort of motives growing out of the beauty of the theory, the love of truth for its own sake, and the desire to understand better the environment in which we live. For this reason we may be sure that workers will be attracted to this subject and that it will have a great development, notwithstanding its inherent difficulty. We shall find after all that much of it is elegant and many of its complexities will disappear in the light of the leading results which we shall attain.

Other progress in the same direction and under like impulses has been realized recently in the development of a theory of functions of curves and spaces. Again it is the physical considerations which have forced the problem upon our attention. This time it is the mathematical physicist who has formulated the new problem and laid the foundations of the consequent theory. If he seeks to study the potential due to an electric current in a fine wire—to take a simple case—it is clear that he has to do with a quantity which depends upon the shape of the wire and is varied by changes in the relative position of its parts. It turns out that there is so much of novelty connected with this new type of functions that some of the fundamental notions, such as that of derivative, for instance, are to be defined in a way not at first obvious. We are thus forced to a fresh analysis of the basic ideas of function theory in the light of a new body of material in which they are to find their use. This will certainly lead to a deeper understanding of these ideas and a more comprehensive view of the body of truth in which they are significant. To an unusual degree our curiosity is piqued to know the lay of the ground here and the direction in which the subject will develop. Already we are assured of its value owing

to its many connections; and the concourse of all fundamental motives in a marked degree assures us of workers for the field and consequent progress of far-reaching character.

It is desirable for us to consider also the distribution of motive at certain periods of great mathematical advances in the more remote past. If we choose typical instances we shall be able to get some conception of the change of emphasis in motive and shall be able to see how the earlier is related to the more recent as put in evidence by the instances which we have just examined.

So far as mathematics was developed at all among the Egyptians, it is clear that it was done in a crude way and essentially for its immediate practical uses. Such a spirit could not release a penetrating study and the consequent insight. It remained for the Greeks to rise to the higher motives associated with the love of truth and beauty and to lay the broad foundations of the science in the spirit which animates it to the present day. Due either to revulsion from the short-sighted vision of the Egyptians or (more likely) to the temperament of the Greeks themselves, the latter too largely ignored mathematical science in its aspect of usefulness in understanding the environment, giving their attention almost entirely to other matters. The new point of view brought with it wonderful advances, but involved also certain elements to stand in the way of a continued and unbroken development.

The discovery of the existence of irrational magnitudes marks a significant event in the intellectual history of mankind. It was a matter of grave concern in the philosophical system of the Pythagoreans, and for a long time they kept to themselves the awful and astonishing secret. In pursuing our study of these quantities (even down to the present day) we have been actuated primarily by the love of truth and beauty. No actual measurements of objects can reveal the presence of these irrational quantities, though it is easy to satisfy ourselves logically of their ideal existence. Only a part of our general motives are operative in their study.

The establishment of a vital and close connection between algebra and geometry was, in the mind of Descartes, a part of his search for a universal mathematical science which was to be only the prelude of a universal science of an all-embracing character. It grew out of a love of truth for its own sake. In geometry we seem generally to have emphasized this motive. It was so with the ancient Greeks, with Descartes, with the rise of modern pure geometrical methods, and with much of the recent development of the geometrical sciences. It is due to the inherent nature of this field of thought.

In the rise and development of the infinitesimal calculus we have a different state of affairs. Here there is scope for the vital activity of all three classes of motives, and they have ever been conjointly in evidence. This is due in part to the extreme breadth of reach of the fundamental ideas of the calculus and in part to its peculiar fitness for the interpretation of motion, the basic element in terms of which we seek to interpret natural phenomena. What grows out of the calculus and is intimately related to it is the most characteristic portion of modern mathematics and is primarily what gives to it its large measure of importance. It is significant that it is also just the portion of modern mathematics in which our three classes of motives operate in the most nearly balanced proportion.

In the theory of functions of a complex variable, which is essentially an outgrowth and extension of the infinitesimal calculus, we have a field of truth which is rich in extent, in beauty, and in the quality of furnishing a means to interpret natural phenomena. In its initial rise, in its main features of interest, in the introduction of new ideas into it, and in its widening ramifications, we find constantly that activity which grows out of an intimate blending of the three general classes of motives. It is the choice part of the most typical field of mathematical activity and illustrates beautifully the proper union and emphasis of motives.

The foregoing analysis of the work of a few mathematicians and of the distribution of motive in the development of certain topics brings to notice three facts which are significant in this study: in the more recent mathematical work we have a definite tendency toward a more nearly equal emphasis on the three general classes of motives than is to be found in the earlier stages; this tendency is most marked in the more characteristic portions of modern mathematics; and this is true particularly in the case of the greater recent developments, especially of those in connection with broad general conceptions such as are present (for instance) in the theory of functions of an infinite number of variables and of functions of curves and spaces. This doubtless reveals a growing necessity incident to the greater complexity of the problems and the larger body of known truth on which the new discoveries must rest.

In our brief survey we have noticed how the greatest mathematical workers and the most important fields of mathematical thought have had an intimate connection with the interpretation of natural phenomena. England and France and Germany are the countries in which the most important and preëminent mathematical progress has been

maintained for a long period of time coming up to the present. They have had workers actuated by all three of our fundamental classes of motives, and a fairly well-balanced emphasis has been maintained among them. The cooperation of each with the others seems to have been essential to the progress effected. So far as mathematical research is concerned, the English have leaned strongly toward its practical aspects, so much so, in fact, that they have suffered somewhat in their contributions; but in the last years there has been a growing tendency among them to correct the evil. In Germany all motives have operated strongly. Owing to the fact that the German university system teaches not only knowledge, but also research, no other country can show so many individual workers nor such a tendency to congregate into schools. Naturally, this is associated with much attention to minor problems and the doing of a large proportion of the drudgery incident to scientific progress. Fruitful ideas do not arise there as spontaneously as in some other countries, but no other people have shown a greater genius for developing the detail in connection with a leading fundamental idea once introduced. Mathematical progress in France during the past century has been of a most pleasing sort. All motives have worked in beautiful cooperation, and spontaneity of effort has led to the creation of many fundamental concepts. Here we see the best balance among the various motives and at the same time the most steady stream of progress. Each generation has been effective in a marvelous degree, and their labors have not only enriched their own discoveries, but have also fructified mathematical thought throughout the world. Their experience and success seem to counsel the holding of a just balance among the three fundamental classes of motives.

So far, in America, we have realized progress in the pursuit of mathematical truth for truth's sake and in the pursuit of high aesthetic satisfactions; but we have hardly realized anything in mathematics from the pursuit of means for interpreting and understanding our environment. This is strange in view of the distinctly practical turn of our people as a whole and suggests the opinion that we probably have an unused reservoir of strength which might become effective in the progress of applied mathematics. It is not to be supposed that a people of a practical turn of mind can produce mathematicians interested in the pursuit of ideal truth and of aesthetic satisfactions and yet not have a source of strength for the development of the more practical aspects of the science. Some of our studies up to the present are adjacent to the field of applications; others are far removed from it; but very little of all that our mathematicians have done lies in the direction of a better understanding of natural phenomena.

We cannot expect to maintain healthy progress by an unproportioned and one-sided development such as has characterized our work to date. Fortunately there is now a rising interest in America in applied mathematics. This should be developed and be guided into the best channels so that the work ultimately shall become of vital and far-reaching importance. By the nature of their previous work some of our mathematicians are definitely excluded from a leading part in this new development. They have devoted themselves to those fields of investigation which are far removed from the sciences of natural phenomena and therefore cannot turn their knowledge and experience to use in this direction. It is no cause for regret that this is so. In order to maintain as a people a balanced emphasis on motives, we need to have individuals in which each fundamental class separately is dominant. But there are those whose labors have already led them to the borderland where pure and applied mathematics have a common boundary line. Some of these, at least, may step over into the adjacent field. For them to do so would seem to afford us our best and readiest means of correcting the patent defect in our mathematical progress. We have already a young national tradition of high ideals in pure mathematics; let us as soon as possible realize the creation of a worthy tradition in applied mathematics.

A survey of the character of our contributions up to the present would probably suggest that our best point of entry is into the field of celestial mechanics, where in fact we have already done something, or into the theory of the partial differential equations of physics. Our previous labors in pure mathematics seem to have furnished us with tools suitable for use in either of these fields.

Every living science has two existences: one of them is objective, as in the body of scientific literature; the other is subjective, as in the minds of thoughtful persons and students of the particular science. The first is like a material body; the second is like the spirit. The first is enduring, like a stone or a mountain; the second is like a living organism, delicate in structure and highly susceptible to environment. Both of these existences are necessary to the progress of a science; indeed, necessary to its continued existence. The primary business of the researcher is to afford science its objective existence in the body of scientific literature. It is the business of the teacher to see that science has the second type of existence. Modern mathematics is now the herit-

age of a select few. We ought to make its great cultural elements a common property of cultivated people. But now it is notorious that they are unashamedly ignorant of this science. Nor is the fault to be laid entirely or even primarily at their door. We teach too much the mechanical aspect of mathematical reckoning and emphasize too little the great basic and fundamental notions which give to the science its vitality.

In each generation the most important labor that can be done is that expended in the creation or discovery of new truth. But this fact must not prevent our realization that the heritage of the past is to be preserved intact and transmitted to the future, not dead as in books, but living as in the minds of men and women. Not only must the line of progress be unbroken, but many collateral branches must run out in all directions into the body of society, where scientific truth may bear fruit for the nourishment of mankind. A few persons will not suffice in this work of disseminating truth; many must be provided if the truth is to be vital in the lives of our people generally.

As far as possible each individual should give his life to labors in which his spirit delights. This is necessary to the higher sort of intellectual achievement as well as to happiness. Unless one rejoices in the realization of the general motives already treated there is no compelling reason why he should devote himself to mathematics; at most he can be only a weary plodder, whatever station he takes. Again, unless the matter of mathematical truth in its broader aspects exercises a deep influence over his meditations, his labor in that field cannot be particularly useful. To devote himself to it would be to waste his life, to spend in fruitless endeavor the energy which might be valuable if employed in more congenial pursuits. But the enthusiastic man or woman of merely moderate training has a place of importance in making mathematics live in the lives of the young, and thence in the lives of the older. This is a labor in which one may take delight and through which one may project an influence into the future that shall work for permanent good.

A few can go further and render a more vital sort of service. A mere modicum of creative work of fair quality is of great value in the way of increasing the vitality of the teacher. The fact that he has created will illuminate the subject for him and give a different color to his teaching. His emphasis becomes better proportioned to the relative importance of the various topics and he is able to light his subject with the glow of personal fire and touch.

A significant measure of research is particularly important in the case of the teacher who prepares textbooks for use in the more advanced undergraduate courses and for the first year of graduate work. It is unfortunate when embryo mathematicians are led over the basic portions of fundamental disciplines through the guidance of a dead exposition, such as will usually emanate from one who has not himself advanced the subject in question or made use of its main ideas or results in some researches of his own. In America we stand now in danger of an increased number of these expositions lacking vitality. This is due to the concurrence of two or three causes. We have now a keen realization of the fact that there is among us a dearth of elementary expositions of the fundamental subjects. Our research men are engaged so zealously in their own investigations that they have usually not taken the time to prepare expository treatments; but this is an evil which seems to be in a state of progressive correction. In many of our institutions—especially among those which have lately aspired to positions of the higher importance—there is a peculiar (and sometimes an even dangerous) pressure upon the members of the faculty to produce publications of a professional sort. Some of the persons involved have not the training or the aptitude for research. There is a danger that an increasing number of these will seek an outlet for their activity in the production of textbooks. Already we have too much of this, so far as the more elementary texts are concerned. Let us hope that we shall be able to prevent the spread of the evil to the field next higher. One satisfactory way of offsetting this danger would be afforded by a greater willingness on the part of our research men to prepare introductory treatments, each one in his own field.

It is only when one is able to devote a large share of his energy to research and is successful in the creation or discovery of important new truth that he may rejoice in the fullest glow of delight through a realization in himself of all three general classes of motives. However important the work of instruction may be in itself, and however far-reaching its stream of influence flowing in hidden ways in the minds of men and women, it cannot be placed in the same category with that creative work which guides instructor and student alike and teaches generations what to think. He who discovers a fact or makes known a new law of nature or adds a novel beauty to truth in any way makes every one of us his debtor. How beautiful upon the highway are the feet of him who comes bringing in his hands the gift of a new truth to mankind!

# The Internal Constitution of the Stars*

PROFESSOR A. S. EDDINGTON
*President of the Mathematical and Physical Science Section*

THERE is another line of astronomical evidence which appears to show more definitely that the evolution of the stars proceeds far more slowly than the contraction hypothesis allows; and perhaps it may ultimately enable us to measure the true rate of progress. There are certain stars, known as Cepheid variables, which undergo a regular fluctuation of light of a characteristic kind, generally with a period of a few days. This light change is *not* due to eclipse. Moreover, the color quality of the light changes between maximum and minimum, evidently pointing to a periodic change in the physical condition of the star. Although these objects were formerly thought to be double stars, it now seems clear that this was a misinterpretation of the spectroscopic evidence. There is, in fact, no room for the hypothetical companion star; the orbit is so small that we should have to place it inside the principal star. Everything points to the period of the light pulsation being something intrinsic in the star; and the hypothesis advocated by Shapley, that it represents a mechanical pulsation of the star, seems to be the most plausible. I have already mentioned that the observed period does in fact agree with the calculated period of mechanical pulsation, so that the pulsation explanation survives one fairly stringent test. But whatever the cause of the variability, whether pulsation or rotation, provided only that it is intrinsic in the star, and not forced from outside, the density must be the leading factor in determining the period. If the star is contrasting so that its density changes appreciably, the period cannot remain constant. Now, on the contraction hypothesis, the change of density must amount to at least 1 per cent in forty years. (I give the figures for $\delta$ Cephei, the best-known variable of this class.) The corresponding change of period should be very easily detectable. For $\delta$ Cephei the period ought to decrease 40 seconds annually.

Now, $\delta$ Cephei has been under careful observation since 1785, and it is known that the change of period, if any, must be very small. S. Chandler found a decrease of period of 1/20 second per annum, and in a recent investigation E. Hertzsprung has found a decrease of 1/10 second per annum. The evidence that there is any decrease at all rests almost entirely on the earliest observations made before 1800, so that it is not very certain; but in any case the evolution is proceeding at not more than 1/400 of the rate required by the contraction hypothesis. There must at this stage of the evolution of the star be some other source of energy which prolongs the life of the star four hundredfold. The time scale so enlarged would suffice for practically all reasonable demands.

I hope the dilemma is plain. Either we must admit that whilst the density changes 1 per cent a certain period intrinsic in the star can change no more than 1/800 of 1 per cent, or we must give up the contraction hypothesis.

If the contraction theory were proposed today as a novel hypothesis, I do not think it would stand the smallest chance of acceptance. From all sides—biology, geology, physics, astronomy—it would be objected that the suggested source of energy was hopelessly inadequate to provide the heat spent during the necessary time of evolution; and, so far as it is possible to interpret observational evidence confidently, the theory would be held to be definitely negatived. Only the inertia of tradition keeps the contraction hypothesis alive—or, rather, not alive, but an unburied corpse. But if we decided to inter the corpse, let us frankly recognize the position in which we are left. A star is drawing on some vast reservoir of energy by means unknown to us. This reservoir can scarcely be other than the subatomic energy which, it is known, exists abundantly in all matter; we sometimes dream that man will one day learn how to release it and use it for his service. The store is well-nigh inexhaustible, if only it could be tapped. There is sufficient in the sun to maintain its output of heat for 15 billion years.

Certain physical investigations in the past year

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make it probable to my mind that some portion of this subatomic energy is actually being set free in the stars. F. W. Aston's experiments seem to leave no room for doubt that all the elements are constituted out of hydrogen atoms bound together with negative electrons. The nucleus of the helium atom, for example, consists of 4 hydrogen atoms bound with 2 electrons. But Aston has further shown conclusively that the mass of the helium atom is less than the sum of the masses of the 4 hydrogen atoms which enter into it; and in this at any rate the chemists agree with him. There is a loss of mass in the synthesis amounting to about 1 part in 120, the atomic weight of hydrogen being 1.008 and that of helium just 4. I shall not dwell on his beautiful proof of this. Now mass cannot be annihilated, and the deficit can only represent the mass of the electrical energy set free in the transmutation. We can therefore at once calculate the quantity of energy liberated when helium is made out of hydrogen. If 5 per cent of a star's mass consists initially of hydrogen atoms, which are gradually being combined to form more complex elements, the total heat liberated will more than suffice for our demands, and we need look no further for the source of a star's energy.

But is it possible to admit that such a transmutation is occurring? It is difficult to assert, but perhaps more difficult to deny, that this is going on. Sir Ernest Rutherford has recently been breaking down the atoms of oxygen and nitrogen, driving out an isotope of helium from them; and what is possible in the Cavendish Laboratory may not be too difficult in the sun. I think that the suspicion has been generally entertained that the stars are the crucibles in which the lighter atoms which abound in the nebulae are compounded into more complex elements. In the stars matter has its preliminary brewing to prepare the greater variety of elements which are needed for a world of life. The radioactive elements must have been formed at no very distant date; and their synthesis, unlike the generation of helium from hydrogen, is endothermic. If combinations requiring the addition of energy can occur in the stars, combinations which liberate energy ought not to be impossible.

We need not bind ourselves to the formation of helium from hydrogen as the sole reaction which supplies the energy, although it would seem that the further stages in building up the elements involve much less liberation, and sometimes even absorption, of energy. It is a question of accurate measurement of the deviations of atomic weights from integers, and up to the present hydrogen is the only element for which Mr. Aston has been able to detect the deviation. No doubt we shall learn more about the possibilities in due time. The position may be summarized in these terms: the atoms of all elements are built of hydrogen atoms bound together, and presumably have at one time been formed from hydrogen; the interior of a star seems as likely a place as any for the evolution to have occurred; whenever it did occur a great amount of energy must have been set free; in a star a vast quantity of energy is being set free which is hitherto unaccounted for. You may draw a conclusion if you like.

If, indeed, the subatomic energy in the stars is being freely used to maintain their great furnaces, it seems to bring a little nearer to fulfilment our dream of controlling this latent power for the well-being of the human race—or for its suicide.

So far as the immediate needs of astronomy are concerned, it is not of any great consequence whether in this suggestion we have actually laid a finger on the true source of the heat. It is sufficient if the discussion opens our eyes to the wider possibilities. We can get rid of the obsession that there is no other conceivable supply besides contraction, but we need not again cramp ourselves by adopting prematurely what is perhaps a still wilder guess. Rather, we should admit that the source is not certainly known, and seek for any possible astronomical evidence which may help to define its necessary character. One piece of evidence of this kind may be worth mentioning. It seems clear that it must be the high temperature inside the stars which determines the liberation of energy, as H. N. Russell has pointed out. If so, the supply may come mainly from the hottest region at the center. I have already stated that the general uniformity of the opacity of the stars is much more easily intelligible if it depends on scattering rather than on true absorption; but it did not seem possible to reconcile the deduced stellar opacity with the theoretical scattering coefficient. Within reasonable limits it makes no great difference in our calculations at what parts of the star the heat energy is supplied, and it was assumed that it comes more or less evenly from all parts, as would be the case on the contraction theory. The possibility was scarcely contemplated that the energy is supplied entirely in a restricted region around the center. Now, the more concentrated the supply, the lower is the opacity requisite to account for the observed radiation. I have not made any detailed calculations, but it seems possible that for a sufficiently concentrated source the deduced and the theoretical coefficients could be made to agree, and there does not seem to be any other way of accomplishing this. Con-

versely, we might perhaps argue that the present discrepancy of the coefficients shows that the energy supply is not spread out in the way required by the contraction hypothesis, but belongs to some new source only available at the hottest, central part of the star.

I should not be surprised if it is whispered that this address has at times verged on being a little bit speculative; perhaps some outspoken friend may bluntly say that it has been highly speculative from beginning to end. I wonder what is the touchstone by which we may test the legitimate development of scientific theory and reject the idly speculative. We all know of theories which the scientific mind instinctively rejects as fruitless guesses; but it is difficult to specify their exact defect or to supply a rule which will show us when we ourselves do err. It is often supposed that to speculate and to make hypotheses are the same thing; but more often they are opposed. It is when we let our thoughts stray outside venerable, but sometimes insecure, hypotheses that we are said to speculate. Hypothesis limits speculation. Moreover, distrust of speculation often serves as a cover for loose thinking; wild ideas take anchorage in our minds and influence our outlook; whilst it is considered too speculative to subject them to the scientific scrutiny which would exorcise them.

If we are not content with the dull accumulation of experimental facts, if we make any deductions or generalizations, if we seek for any theory to guide us, some degree of speculation cannot be avoided. Some will prefer to take the interpretation which seems to be most immediately indicated and at once adopt that as a hypothesis; others will rather seek to explore and classify the widest possibilities which are not definitely inconsistent with the facts. Either choice has its dangers; the first may be too narrow a view and lead progress into a cul-de-sac; the second may be so broad that it is useless as a guide, and diverges indefinitely from experimental knowledge. When this last case happens, it must be concluded that the knowledge is not yet ripe for theoretical treatment and that speculation is premature. The time when speculative theory and observational research may profitably go hand in hand is when the possibilities, or at any rate the probabilities, can be narrowed down by experiment, and the theory can indicate the tests by which the remaining wrong paths may be blocked up one by one.

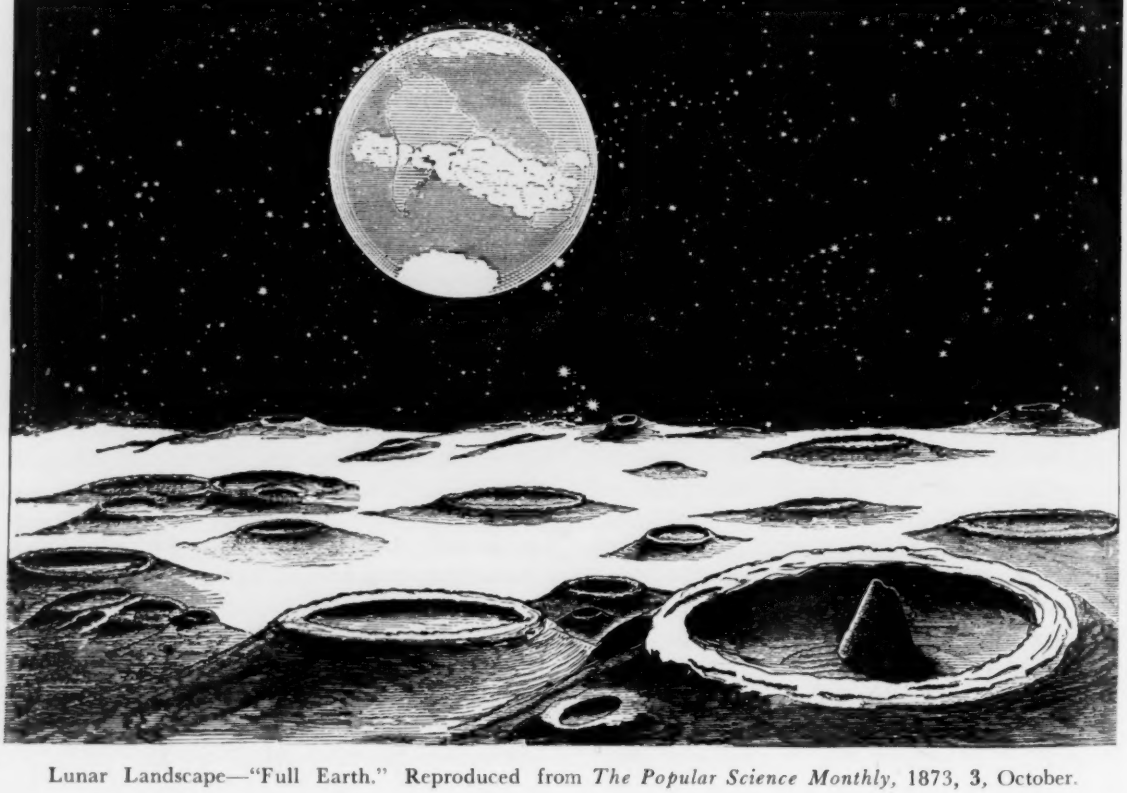
The mathematical physicist is in a position of peculiar difficulty. He may work out the behavior of an ideal model of material with specifically defined properties, obeying mathematically exact laws, and so far his work is unimpeachable. It is no more speculative than the binomial theorem. But when he claims a serious interest for his toy, when he suggests that his model is like something going on in nature, he inevitably begins to speculate. Is the actual body really like the ideal model? May not other unknown conditions intervene? He cannot be sure, but he cannot suppress the comparison; for it is by looking continually to nature that he is guided in his choice of a subject. A common fault, to which he must often plead guilty, is to use for the comparison data over which the more experienced observer shakes his head; they are too insecure to build extensively upon. Yet, even in this, theory may help observation by showing the kind of data which it is especially important to improve.

I think that the more idle kinds of speculation will be avoided if the investigation is conducted from the right point of view. When the properties of an ideal model have been worked out by rigorous mathematics, all the underlying assumptions being clearly understood, then it becomes possible to say that such and such properties and laws lead precisely to such and such effects. If any other disregarded factors are present, they should now betray themselves when a comparison is made with nature. There is no need for disappointment at the failure of the model to give perfect agreement with observation; it has served its purpose, for it has distinguished what are the features of the actual phenomena which require new conditions for their explanation. A general preliminary agreement with observation is necessary, otherwise the model is hopeless; not that it is necessarily wrong so far as it goes, but it has evidently put the less essential properties foremost. We have been pulling at the wrong end of the tangle, which has to be unraveled by a different approach. But after a general agreement with observation is established, and the tangle begins to loosen, we should always make ready for the next knot. I suppose that the applied mathematician whose theory has just passed one still more stringent test by observation ought not to feel satisfaction, but rather disappointment—"Foiled again! This time I *had* hoped to find a discordance which would throw light on the points where my model could be improved." Perhaps that is a counsel of perfection; I own that I have never felt very keenly a disappointment of this kind.

Our model of nature should not be like a building—a handsome structure for the populace to admire, until in the course of time someone takes away a cornerstone and the edifice comes toppling down. It should be like an engine with movable parts. We need not fix the position of any one lever; that is to be adjusted from time to time as the

latest observations indicate. The aim of the theorist is to know the train of wheels which the lever sets in motion—that binding of the parts which is the soul of the engine.

In ancient days two aviators procured to themselves wings. Daedalus flew safely through the middle air across the sea, and was duly honored on his landing. Young Icarus soared upwards toward the sun till the wax melted which bound his wings, and his flight ended in fiasco. In weighing their achievements, perhaps there is something to be said for Icarus. The classic authorities tell us that he was only "doing a stunt," but I prefer to think of him as the man who certainly brought to light a constructional defect in the flying machines of his day. So, too, in science. Cautious Daedalus will apply his theories where he feels most confident they will safely go; but by his excess of caution their hidden weaknesses cannot be brought to light. Icarus will strain his theories to the breaking point till the weak joints gape. For a spectacular stunt? Perhaps partly; he is often very human. But if he is not yet destined to reach the sun and solve for all time the riddle of its constitution, yet he may hope to learn from his journey some hints to build a better machine.



Lunar Landscape—"Full Earth." Reproduced from *The Popular Science Monthly*, 1873, **3**, October.

# On the History of Physiology and Some of Its Lessons*

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TO DEAL adequately with the history of a science, such as physiology, one should be both a scientist and a historian. But at present the two functions are almost never found in one person. History, as ordinarily taught and practiced, is certainly the least scientific of all branches of knowledge. As a great scientist, Wilhelm Ostwald, in his lectures before the Rice Institute on the "Organization of Knowledge," has truly said, "the essence of knowledge organized as a science is the attainment of the power to control the future, or at least foretell its course."

Prophecy as practiced in science is not mere fortunetelling to satisfy an idle curiosity, but guidance and direction for our acts and work. Astronomy foretells for the sailor the hours of the tides, the positions of the stars, and for us all the occurrence of such events as eclipses and the passage of meteors. Someday it may even foretell the weather —and years ahead.

The prophecies of chemistry take the form of such conditional propositions as this: If you mix certain substances in certain proportions under certain conditions, you will get such an amount of such a product—be it rubber or leather or steel or dye. Physics likewise foretells that if we induce certain steam pressures in engines of a certain design, we shall obtain such an amount of power. Economics foretells that a rise of prices will result if too much paper money is printed. It is this power of conditional prophecy which is used to run trolley cars, and to light our homes and halls, and in general to make material civilization possible.

The history of science according to this view should be, therefore, not merely a chronicle of discovery, but an analysis of the relations of ideas and of methods to progress and the application of the conceptions thus gained to guide us in present and future work; for the fundamental idea of science—the idea which historians have not yet grasped, nor yet recognize as a duty even with bleeding Europe before their eyes—is that in this world of nature and of man certain sequences of events continually recur, but never twice strictly and exactly alike, and that the business of organized knowledge is to define the principle of repetition, to show the extent and the reasons for the variations, and to suggest how mankind may mould the world to culture and civilization out of the "original sin" of nature.

When we face the topic with this idea in mind, the first consideration which presents itself is that in what we today call a distinct science, such as physiology, physics, or chemistry, we are speaking in reality of a strictly modern and ephemeral thing. Each has its roots and ancestors in the past but, so to speak, each makes itself out of the materials inherited from the past and ennobles its own ancestors. Moreover, each will probably in the future be divided up, its name transferred, or even forgotten along with astrology, alchemy, and heraldry, and its various components recombined in other subjects whose names have not yet been even invented.

Such a subject as the physiology of A.D. 1922 certainly did not exist in 1822, any more than you and I existed then; and probably by 2022 our physiology will be with you and me and "yesterday's 7,000 years."

Before we go back to the past ages to trace the beginnings of physiology, we must define what particular conceptions and objects in the broad current of science we understand in A.D. 1922 under the word physiology. We understand two things, either or both of which enter into every problem in any of the various fields of physiology. May I illustrate by quoting from the recent Silliman lectures here by Professor Krogh? He was speaking of the physiology of capillaries and he said:

* The third of a series of lectures on the History of Medicine, by a number of lecturers, under the auspices of the Gamma Alpha Society of Yale University.

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As is generally the case in physiological research, we have a double purpose in studying the reactions of capillaries to physical and chemical stimuli: we want to find out the mechanism (in the broadest sense of that term) of every single reaction studied, and we want to find out also the meaning, the part played by the reactions in the delicate regulations by which the organism and the organs are adapted to the ever-changing environmental conditions.

The "mechanism" and the "meaning" of living creatures and their organs, these are the questions of physiology. It is because the mechanisms of living things are, as we now know, fundamentally the same as those of physics and chemistry that we physiologists partake of the same heritage as physicists and chemists. We claim as part of the heritage of physiology Archimedes, with his ideas of levers and of specific gravity; Torricelli and his conceptions of the pressure of a column of liquid; Lavoisier and Liebig and all those great names which are associated with the development of our ideas as to the conservation of energy and the indestructibility of matter. The very core of physiology lies in what we term metabolism, and the chemists term transformations of matter with no change of weight, and the physicists formulate under the laws of thermodynamics. From a mechanistic standpoint, Lavoisier was the greatest physiologist who ever lived, and yet the chemists would certainly claim him as their own. But Lavoisier's work cannot be divided into a physiological part and a chemical part. No, it was all chemistry and practically all physiology as well. Science is one, and I will venture the guess that perhaps one hundred years from now in a lecture here on the history of physiology, the lecturer may say, and say truly, that in the latter part of the nineteenth century there lived and worked in New Haven a physiologist as great as Lavoisier, a man who taught those principles of equilibrium in such chemical systems as the humors and cells of living organisms, upon which the ultimate explanation of life must rest—Willard Gibbs, a mathematical physicist.

Perhaps, someday in an infinitely distant future, physiology will thus be converted into a branch of mathematical mechanics. But when a problem of mechanism is solved, we still have before us, as Professor Krogh said in the passage which I quoted, the problem of the meaning—that is, as we say in physiology, the function of these mechanisms. Here we part company from physicists and chemists, for they utterly reject, or at least neglect, teleology nowadays. They can afford to; but in dealing with living things we must guide and check our work by teleological considerations or we may go far astray. As Aristotle put it 2,500 years ago: "In dealing with respiration, we must show that this and that part of the process is necessitated by this and that other stage of it"—that is, its mechanism; and we must also "show that it takes place for such or such a final object."

ANTOINE LAURENT LAVOISIER (1743–1794)
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On the other hand, the standpoint of modern physical science regarding teleology is summed up in an anathema composed by Francis Bacon about 400 years ago:

To say that the hairs of the eyelids are for a quickset and fence about the sight; or that the firmness of the skins and hides of living creatures is to defend them from the extremities of heat or cold; or that the bones are for columns or beams whereupon the frames of the bodies of living creatures are built; or that the leaves of trees are for protection of the fruit; or that the clouds are for watering of the earth; or that the solidness of the earth is for the station and mansion of living creatures, and the like is well enquired and collected in Metaphysic; but in Physic they are impertinent.

For chemistry and for physics, and doubtless also for biochemistry and biophysics, such conceptions are today "impertinent." But the formulation of such conceptions regarding the living body is the very essence of physiology proper, for the word "physiology" is derived from the Greek words *logos* and *phusis* or *physis*. It means the principles of na-

ture; but *physis,* as Hippocrates used the word, means far more than nature: it is "the something working toward an end," the something which von Leiden recognized when he said, "If you can only keep your patient alive long enough he will generally get well of himself." And here let me point out that no mere mechanism, such as an automobile, ever recovers by merely being left alone. In my experience even a broken spark plug always requires a surgical operation.

Now let us make a rapid survey of the great contributions to the problems both of mechanism and of meaning and see how they are related.

The mammalian circulation, and especially the heart, have probably been the object of more investigation than any other topic in science. Aristotle, whom Osler speaks of as the founder of physiology, as well as of comparative anatomy, zoology, botany, and some other sciences, recognized the heart as the central organ controlling the circulation, the seat of vitality, the place where the blood was finally elaborated and impregnated with animal heat. Aristotle did not distinguish between arteries and veins, but recognized that from the blood vessels throughout the body "the nutriment oozes like water in unbaked pottery."

A clearer view of some points, but not so on others, was reached by Praxagoras, who taught in the celebrated University at Alexandria a quarter of a century or so after Aristotle. He recognized that only the arteries pulsate, but he taught that these vessels were really air tubes, only the veins containing blood. Hence he applied the word "artery," which we still use, but which literally means an air tube like the windpipe, the trachea, and bronchi. It was an odd mistake, but perhaps the following incident may suggest how Praxagoras happened to make it: Some years ago in connection with a lecture, I was demonstrating the anatomical relations of the heart and lungs of a big dead Newfoundland dog, when one of my hearers, a burly, black-bearded man, pointed to the aorta and asked what it was. I told him it was the great artery. "But," said he, "it never contains blood, for I have cut up many animals and have always found in it only air or a little bloody foam." Of course I was curious to know how he came to cut up many animals; and found that he was assistant rabbi of an Orthodox synagogue and that he koshered cattle—that is, cut their throats. Apparently the dying gasps of an exsanguinated animal fill the aorta with air, as the rabbi, and perhaps Praxagoras also, found, for the Bible tells us that koshering started in or near Egypt. Failure to appreciate the abnormal conditions in experiments on animals has led many modern investigators to almost equally erroneous conclusions in some of our problems.

The Alexandrians also described the valves of the heart, but it was 400 years later, that is, in the second century A.D., before Galen, the great Roman physiologist and physician, proved that the arteries contain blood. He did it by an experiment, not on a dead body, but on a living animal. He placed two ligatures a little distance apart upon an artery. "Now," said he, "if I cut the vessel, spiritus [or, as we would say, air] will come out, if Praxagoras is right; but blood if I am right in holding that the arteries are really blood vessels." Then he cut a nick in the vessel—just as every student doing a blood pressure experiment does nowadays to insert a cannula—and of course there was a little spurt of blood. But there he stopped, and it was 1,400 years before William Harvey, an Englishman, physician to King James the 1st, discovered and proved by ingenious experiments that the heart is a pump and that the blood runs in a circle.

It is extremely difficult for us to form a clear conception of some of the medieval beliefs regarding the living body and scientific matters in general, and I shall merely abbreviate here the account which Sir Michael Foster gives of the Galenic physiology.

The parts of the food absorbed from the alimentary canal were carried by the portal vein to the liver, and were there converted into blood, or rather the blood was there enriched with nutriment, or, as it was termed, "natural spirits." This crude blood, passed by the *vena cava* to the right side of the heart, and some of it filtered from the right ventricle through innumerable invisible pores in the septum into the left ventricle. As the heart expanded in its beat, it drew air from the lungs into the left ventricle. This mixture of blood and air by the help of the heat, which was innate in the heart, which was placed there as the source of the heat of the whole body by God in the beginning of life, and which remained there until death, was imbued with "vital spirits," while at the same time the innate heat of the heart itself was tempered and prevented from becoming excessive.

From the right side of the heart there was sent to the body generally along the great veins and also to the lungs a flow, followed by an ebb, of crude blood endued with natural spirits only, blood serving the lower stages of nutrition. From the left side of the heart there took place also along the arteries to all parts of the body a flow, followed by an ebb, of blood endued with vital spirits, and so capable of giving power to the several tissues to exercise their vital functions. It was the blood which the left

heart sent to the lungs through the pulmonary veins which carried those fulginous vapors which, in the fermenting activity giving rise to the vital spirits, were extracted from the crude blood, and were finally discharged into the air in the lungs. The nutriment of the food which had been converted into natural spirits in the liver and transformed into vital spirits in the heart, finally reached the brain by way of the arterial blood and there generated the animal spirits, which, pure and unmixed with blood, were carried along the nerves to bring about muscular movements and other functions of the body.

All this sounds exceedingly crude in its conception of the heart and blood vessels, but it is still cruder in what it omits, namely, in the lack of any conception of the energy-liberating function, metabolism, the mechanism and meaning of respiration, and the relation of the organism to the atmosphere. For all of this, as I have said, we have to wait until Lavoisier founded modern physiology less than 150 years ago.

It was not until near the middle of the sixteenth century that anyone seemed to have grasped the true relation of the heart and lungs; then Michael Servetus, in a great theological work, with physiological illustrations, included a passage in which he announced that the blood, instead of filtering through the walls of the ventricles, is, on the contrary, pumped from the right heart to the lungs and, thus aerated and made bright by the passage, is sent into the left heart; there, apparently, he thought it stopped. It seems odd, almost humorous, that this physiological discovery should have been included in a work on theology, but humor gives place to thought when we recollect that Servetus was burnt at the stake for the religious views and doubtless also in part for the physiological views which his book contained; yet the beliefs of both sorts stated in that book were essentially what all educated people now hold; and the man who brought about his death was Calvin, the great founder of that movement in thought out of which to a large extent Puritan New England and Yale University took their origin. Evidently it is not always best even for very positive people introducing an important new idea to be too hasty in destroying those who differ with them.

Vesalius, the founder of anatomy, who also lived in the sixteenth century, thought it safer as regards the Galenic conception of the heart to speak as follows:

> The septum of the ventricles, composed as I have said of the thickest substance of the heart, abounds on both sides with little pits impressed in it. Of these pits, none, so far at least as can be perceived by the senses, penetrate through from the right into the left ventricle, so that we are driven to wonder at the handiwork of the Almighty by means of which the blood sweats from the right into the left ventricle through passages which escape human vision.

As a reward of this caution, Vesalius escaped serious persecution and died in his bed. Anatomists have always been a quieter, less restless sort of people, with less desire for revolution and changes in the curriculum than have the physiologists.

We come now to William Harvey. There is little need to describe Harvey's conception of the heartbeat, for it is almost word for word contained in every textbook of physiology at the present time. In most points regarding the heart itself this clear, simple-minded, straightforward experimenter attained completeness and perfection. His book, *The Motion of the Heart and Blood in Animals,* is the most delightfully readable of scientific works and, as a good English translation of its original Latin is purchasable in the cheap but serviceable Everyman's Library, every student in any branch of biological science should own and read it. When Harvey is read firsthand, it becomes clearer than any lecturer can explain that it was the lack of knowledge of what we now call energetics and metabolism, physics and chemistry, and the real meaning of respiration which gave to his path its peculiar difficulty and limited his discovery. It is his merit that in the midst of the misconceptions of these matters which he could not and did not correct, he attained truth for his own special topic, the mechanism and meaning of the heart movements. He says:

> When I first gave my mind to vivisections, as a means of discovering the motions and uses of the heart, and sought to discover these from actual inspection, and not from the writings of others, I found the task so truly arduous, so full of difficulties, that I was almost tempted to think with Frascatorius that the motion of the heart was to be comprehended only by God. For I could neither rightly perceive at first when the systole and when the diastole took place, nor when and where dilatation and contraction occurred, by reason of the rapidity of the motion, which in many animals is accomplished in the twinkling of an eye, coming and going like a flash of lightning; so that the systole presented itself to me now from this point, now from that; the diastole the same; and then everything was reversed, the motions occurring, as it seemed, variously and confusedly together.

His most difficult task, as is often the case in a discovery of the first rank, was to refute the errors and misconceptions of the time, and to free his readers from false modes of thought. He says:

> Almost all anatomists, physicians, and philosophers, up to the present time, have supposed with Galen, that the object of the pulse was the same as that of respiration; that is, to cool the blood. But did the arterial pulse and the respiration serve the same ends; did the arteries in

their diastole take air into their cavities, as commonly stated, and in their systole emit fuliginous vapors by the same pores of the flesh and skin; and further did they in the time intermediate between the diastole and the systole, contain air, and at all times either air, or spirits, or fuliginous vapors, what should then be said of Galen's experiment showing that the arteries contain blood?

Having thus given his reasons for believing that the heart performs some function other than that of the chest and lungs in breathing, he proceeds to give the following description, the classic account of the heartbeat:

> At the moment the heart contracts, and when the breast is struck, when in short the organ is in its state of systole, the arteries are dilated, yield a pulse and are in the state of diastole. . . . The arteries, therefore, are distended, because they are filled like sacks or bladders, and not because they expand like bellows. It is in virtue of one and the same cause, therefore, that all the arteries of the body pulsate, namely, the contraction of the left ventricle; in the same way as the pulmonary artery pulsates by the contraction of the right ventricle.

All the points which these quotations assert were demonstrated by Harvey by numerous experiments and keen observations on living animals, on frogs, snakes, pigeons, fish, dogs, and so on, and also by experiments and observations on the veins in the arms of man.

Few figures in science stand out as originative and creative as Harvey, and yet he has been criticized from two almost directly opposite standpoints. The points made are true but do not really decrease his merit. It has been shown on the one hand that the greater part of what Harvey wrote had been thought and suggested by others previously more or less clearly and that he got the germ and possibly even some of the form of his ideas when a student in Italy, for the Italian universities were at that time the living centers of thought in Europe. On the other hand, it has also been shown, particularly by the late Professor J. G. Curtis, who was profoundly versed in the physiology of the Greeks and Romans, that to a considerable extent Harvey himself was as much in the dark regarding the uses of the circulation as any of his contemporaries, and that he fell back upon Aristotle just as they did. Here he gropes and stumbles and fails; and, as Curtis shows, the circulation in Harvey's mind apart from the physical movements of the heart was more Greek than modern. It had in it more of Aristotle than of Carl Ludwig.

There is no doubt that there is truth in both these comments upon Harvey. He drew on the knowledge contained in the literature of his time and was hindered or helped by what it lacked or contained just as we are now. It puts such matters in their true light, however, to notice that the same sort of comments apply to nearly all the great discoverers; for instance, in the case of Christopher Columbus, it has been shown, I believe, both that others had discovered America before him and also that Columbus did not realize that he was discovering America. Neither Columbus nor Harvey nor any other great discoverer can possibly realize the enormous consequences which will grow out of his work, but this fact does not in the least decrease their greatness and the immensity of our debt to them.

Even with all the resources of modern laboratories, the problem of just how much blood the heart discharges at each beat is not yet solved much beyond the point at which Harvey left it.

In Harvey's day or shortly afterward, there lived in Italy two great physiologists, Malpighi and Borelli: intimate friends at first, but later disagreeing, Borelli vented his feelings on his colleague's scientific work in quite modern fashion. Malpighi was chiefly a great anatomist, and especially a histologist, and as such his work has been described in a preceding lecture of this series, but he has this supreme interest for us that he was the first to turn the microscope upon the finer blood vessels of the circulation and thus to see the marvelous picture of the blood pulsating in arterioles, its corpuscles squeezing through capillaries, to be reassembled in the central column of the slow and steady stream of the venules. It is most significant that this discovery depended upon the use of the then new physical instrument of precision, the microscope.

Borelli was one of the first to accept Harvey's teaching, and he explained more fully than Harvey had done the mechanics of the pulse. He recognized the part which the elasticity of the arteries plays in converting the intermittent discharge of the heart into a steady flow and first expressed the idea of the peripheral resistance of the circulation. Here again we find the association of physics and physiology. As a student Borelli had studied the writings and for a time had listened to the lectures of Galileo, the man who first recognized that the world revolves about the sun and who defined gravity and other physical conceptions. Borelli had associated with and been influenced also by Torricelli, to whom we owe our modern conceptions of atmospheric pressure and of the nature of a vacuum and the barometer.

It was in such an atmosphere that Borelli worked; a mathematician, a physicist, and a physiologist. His greatest work was on animal motion. He treated the bones as levers and the muscles as motors and worked out correctly many of the mechanical movements of the body and limbs, as well as of the viscera. Much of what he said is as true

today as it was when he wrote it. He also dealt the death blow to that idea of a "spiritus" or air, which was supposed to be blown by the brain through the nerves, then believed to be minute tubes, to the muscles, inflating them and thus causing their contraction. His crucial experiment, which, as he expressed it, "does away with all this nonsense about spiritus," he describes thus:

> When the muscles of a living animal are divided lengthwise, while the animal is submerged under water, and in consequence of the pain is struggling violently, in the midst of such great copius fervor and ebullition of the supposed spirituous gas which would thereby be excited in the muscles, one would expect that innumerable bubbles of gas would burst forth from the wound, and ascend through the water; whereas nothing of the kind takes place.

You will all recognize the soundness of this experimental method of locating an air leak.

About the time that Borelli died in Italy, another remarkable man was born to carry on his work. Born in England, Stephen Hales was a younger son of an English country gentleman and, as was the custom of younger sons, he turned to the church for a living. He studied at Cambridge and obtained a B.A., M.A., and B.D. and later, from Oxford, an honorary D.D. In college he did many dissections and repeated the experiments, the scientific novelties of that day, by which Boyle had been led to the gas law which bears his name. Later Hales lived the life of an active country clergyman, but he seems to have thought the glory of God was best demonstrated by revealing the wonders of nature, for few scientific men of that time have as long or as valuable a list of experimental investigations as has Hales. He was extraordinarily modern-minded, for in practically all his work he used accurate quantitative methods, and his employment of the word "equilibrium" in the following definition of health would be creditable in 1922. He says:

> As the healthy state of the animal principally consists in the maintaining of a due equilibrium between the fluids and solids, it has ever since the important discovery of the circulation of the blood been looked upon as a matter well worth inquiring into to find the force and velocity with which these fluids are impelled; as a likely means to give considerable insight into the animal economy.

Accordingly, he performed the celebrated experiment which bears his name and which he describes as follows:

> In December I caused a mare to be tied down alive on her back; she was 14 hands high and about 14 years of age; had a fistula on her withers; was neither very lean nor yet lusty; having laid open the left crural artery about three inches from her belly, I inserted into it a brass pipe whose bore was one-sixth of an inch in diameter; and to that, by means of another brass pipe which was fitly adapted to it, I fixed a glass tube, of nearly the same diameter which was nine feet in length; then untying the ligature on the artery, the blood rose in the tube eight feet, three inches perpendicular above the left ventricle of the heart . . . when it was at its full height, it would rise and fall at and after each pulse two, three, or four inches. . . .

This was the first accurate measurement of arterial pressure.

Thus far we have talked mainly of the circulation because in this brief summary of the history of physiology, we are omitting mere chronicles of false and misleading ideas and are selecting as the origins of our science those ideas only which were and are true or which have led to truth. Under this rule, there is little to say of the chemical side of physiology down through Greek, Roman, and medieval times. The alchemists were as furtive as our forgers, for alchemy was a crime close to witchcraft, an offense for which one was burnt at the stake. Thus when that amazing figure, Theophrastus Bombast von Hohenheim, also called Phillipus Aurelius, and commonly known as Paracelsus, who is said to be the original of Goethe's Faust, first introduced chemistry to physiology, along with opium, hydrochloric acid, and other new drugs and chemicals, he also brought the mystical idea that over each reaction presides an Archeus, a "spirit called from the vasty deep," to control it. From these Archei our digestive and other ferments are descended, and perhaps the vitamins belong in the same family. It is difficult to find an easily quotable passage to give an idea of what an Archeus was in the mind of an alchemist facing a problem in biochemistry. But a great modern thinker and writer who was also a thoroughly medievally minded man, the late Cardinal Newman, has expressed essentially the same idea in these wonderful words:

He is speaking of his opinions of angels and he says:

> I viewed them not only as the ministers employed by the Creator in the Jewish and Christian dispensations, as we find on the face of Scripture, but as carrying on as Scripture also implies the economy of the visible world. I considered them as the real causes of motion, light and life, and of those elementary principles of the physical universe, which, when offered in their developments to our senses suggest to us the notion of cause and effect, and of what are called the laws of nature. . . . What would be the thoughts of a man who when examining a flower or a herb or a pebble or a ray of light, which he treats as something so beneath him in the scale of existence, suddenly discovered that he was in the presence of some powerful Being, who was hidden behind the visible things he was inspecting, who, though concealing his wise hand, was giving them their beauty, grace and perfection, as being God's instrument for the purpose, nay, whose robe and ornaments those objects were, which he was so eager to analyze. . . . Every breath of air and light and heat, every beautiful prospect, is, as it were, the skirts of their

garments, the waving of the robes of those whose faces see God.

As a further illustration, I am reminded of some words of St. Augustine which I came across in his *Confessions* not long ago and which show the medieval attitude toward scientific investigation. He was discussing the nature of time. Time, he said, may be measured as the duration needed to read a certain number of verses, or to walk a certain distance. What will be its end? he asks. Obviously the Judgment Day. What was its beginning? Obviously the morning of the first day of creation. But what, he asks finally, was God doing before the creation? And he answers by saying that, according to the best authorities, God was then engaged in preparing Hell for those who inquire too closely into his mysteries.

Following Paracelsus came van Helmont, who was half-modern and half-medieval. He invented the word "gas" and to a considerable extent the conception for which it stands: an enormous service, for he described correctly to a great extent the formation of carbon dioxide in the fermentation of grapes, from which idea, as you will see, by slow gradations has come our idea of respiration. Of gastric digestion he said: "If the ferment were only an acid, vinegar alone would be able to transmute a mass of bread and be sufficient for the transformation of all our food." A ferment he described as "a specific gift of vital nature." He says also that the acid chyle passing from the stomach into the duodenum immediately acquires a saline nature, and changes from an acid into a salt, "just as vinegar by the addition of nimium [lead oxide] is changed into an aluminous sweetness." In the intestine a further change in the food materials is brought about, as he expressed it, "through a more excellent vigor of transmutation." He still believed in the Archeus, a sensitive soul, as the controlling agent in all bodily acts; but he was sufficiently modern to try to determine the location of the soul by experimental methods. Thus he made the seat of the Archeus not the brain, but the pylorus of the stomach; and for this he gives three reasons: first, a great emotion is always felt in the stomach; second, a man's head may be blown off by a cannon ball and his heart continues to beat for a short time; but, third, his heart is instantly stopped by a blow in the stomach.

Following van Helmont in the next generation, Francois duBois, or Sylvius, discarded the Archeus and expelled spiritualism from biochemistry. Like a modern biochemist he held that if we had complete knowledge of the chemical reactions between acids and alkalis, life itself would be explained. But he also made just the sort of mistake that a modern biochemist knowing much chemistry and little biology makes: he saw gas bubbles rise in fermenting dough and also in a vessel into which he had put carbonate of lime and hydrochloric acid, so he inferred that the chemistry of the two processes was identical. Sylvius assigned great importance in digestion to saliva, and one of his pupils, DeGraaf, among other important work, was the first to insert a cannual in the duct of Wirsung and to collect pancreatic juice. Both Sylvius and DeGraaf held that the juice was acid.

Space prevents my telling of the contributions and ideas of Stahl, Boerhaave, and the great Haller, who wrote a textbook of physiology in eight large volumes in Latin. One of the most concrete experimental additions to our knowledge of digestion dating from this time was made by Réaumur. He fed a kite, a sort of hawk, with metal tubes with wire nettings over their ends containing meat. When the kite, according to its habit, regurgitated the tubes from its stomach, Réaumur noted that the meat had been dissolved out. The diet proved, however, too much for the kite, and its death ensued. Réaumur then carried on the same sort of experiments on dogs, showing the solvent action of gastric juice, which he was the first to obtain by placing pieces of sponge in the little metal tubes which he gave his animals to swallow, and which he obtained again from their stomachs.

Réaumur's line of experimentation was later continued with much industry and ingenuity by Spallanzani in Italy. But the extent of the knowledge gained is shown by the report which Spallanzani obtained from one of his colleagues, the professor of chemistry, who analyzed for him a sample of gastric juice. The chemist reported that "the fluid contains first pure water, secondly a saponaceous and gelatinous animal substance, thirdly salammoniac, and fourthly an earthy matter like that which exists in all animal fluids. It precipitates silver from nitrous acid and forms luna cornea. This phenomenon might induce us to suppose that common salt exists in the gastric juice; but the salt contained in this fluid is not common salt but salammoniac."

Now we approach the nodal point where the major lines of chemistry, physiology, and physics unite to radiate again into the broad field of modern science. At the very center of the node we find Lavoisier. But first a few words of the men whose work he interpreted.

For the chemist the problem was the nature of combustion; for the physicist, it was the origin of the heat and power liberated; for the physiologist,

it was the meaning of respiration and the source and mode of the heat and power of the living body. That respiration was the very essence of life even the Greeks fully realized, but they had scarcely any conception of the physical character of a gas or the chemical differences between various gases. But let us be modest in our knowledge, for among the students of today an easy comprehension of the nature of gases and their laws is so rare as to be almost a special gift.

It was Robert Boyle who in 1660 took the first great step by showing by means of the air pump that in a vacuum a candle is extinguished and a mouse or sparrow dies. Seven years later Robert Hooke showed that it is not the movements of the chest which maintain life, but the passage of air through the lungs, for after opening the thorax of a dog, and making a number of small holes in the lungs for the air to escape through, he blew a constant strong current of air into the trachea, and life was maintained. At nearly the same time Lower demonstrated that it is the aeration of the blood in the lungs which causes the change of color from the dark maroon of venous to the bright scarlet of arterial blood, and he even sensed the relation of respiration to combustion, for he says of the need of fresh air, "were it not for this, we should breathe as easily in the most filthy prison as among the most delightful pastures;" and again he says: "where a fire burns readily, there can we breathe easily." Lower was an Oxford man.

So also was Mayow, whose attention was caught by the part which nitre plays in giving to gunpowder its power. Following this lead through ingenious experiments, he came to the conclusion "that this air which surrounds us, and which since by its tenuity it escapes the sharpness of our eyes, seems to those who think about it to be an empty space, is impregnated with a certain universal salt, of a nitro-saline nature, that is to say with a vital, fiery, and in the highest degree fermentative spirit." This of course was oxygen. He recognized that burning and breathing were essentially identical and that the part of air essential to life is absorbed by the blood as it passes through the lungs. He almost sensed the nature of carbon dioxide also. But Mayow, Boyle, Lower, and Hooke died and left no successors. The great Oxford school of physiology ceased for nearly 200 years, to be revived brilliantly by Haldane in this same field of respiration in our own day.

In the years following the English school, George Ernest Stahl, working at Weimar, Halle, and Berlin, and making indeed valuable contributions, yet did more harm than good by his celebrated theory of phlogiston. He defined phlogiston as the essence of fire, for he conceived fire as a material thing. When any substance was burned, such as a piece of carbon, or when a metal was oxidized, the process was considered by Stahl to consist in the escape of phlogiston. When an oxide was reduced, phlogiston was, he thought, imparted to the metal. Thus physically phlogiston was a sort of pseudo energy; chemically it was the antithesis of oxygen. This was altogether the most harmful error that has ever occurred in science. It utterly obscured the chemistry and energetics of respiration.

About the middle of the eighteenth century, Joseph Black, professor of chemistry at Edinburgh, identified what he called "fixed air," which, as he found, comes off when limestone is calcined—in other words, carbon dioxide. He also showed that this fixed air is produced by fermentation, by burning charcoal, and that it is given off in the air expired from the lungs. He identified it in each case by the precipitate, or white cloud, which "fixed air" produces when blown through lime water.

About this time also Joseph Priestley first prepared oxygen. He had early found that air made irrespirable by burning a candle in it could be made again respirable by growing a green plant in

JOSEPH PRIESTLEY (1733–1804)
Reproduced from *The Popular Science Monthly*, 1874, 5, August.

it. In 1774 he made oxygen gas by heating oxide of mercury and showed its properties; but he was unable to free his mind from the phlogiston theory. He called breathable air "dephlogisticated air" and air rendered irrespirable by burning a candle in it he called "phlogisticated air." So, too, venous blood he considered to be blood laden with phlogiston which it gave off in the lungs where it came in contact with dephlogisticated air. His whole conception of combustion and of respiration was thus a completely inverted image of the truth.

Meanwhile Scheele, a struggling apothecary but a man of genius, in Sweden, was doing independently the same type of experiments, and expressed his conception of respiration in the suggestion that "the blood and lungs change fire air into acid air."

Now we come to Lavoisier, a master figure in three sciences: chemistry, physiology, and physics.

Lavoisier was a rich tax collector for the French government and as such was connected with the Treasury. Gold is the one thing in respect to which weighing with fine balances is most important, and the association of Lavoisier through tax collecting with the art of weighing accurately was probably of critical importance in guiding his genius. Through the inherent intuitional power of his mind he assumed as no one else had done, and he proved, that matter can neither be produced nor destroyed as it passes from one form to another and that the one chemical property which is indestructible is its weight. He found that a reaction occurring inside a closed glass vessel involves no change of weight, and, as he said, "the usefullness and accuracy of chemistry depend entirely upon the determination of the weights of the ingredients and products both before and after the experiment."

Thus he established the major principle of modern chemistry. With his associate, Laplace, he also constructed an ice calorimeter and determined the amount of ice which was melted when a given amount of carbonaceous material was burned in it. Thus for physics he laid the cornerstone of thermodynamics. For physiology he revealed the association of respiration and animal heat, for he put a guinea pig into the ice calorimeter and found that it produced 224 grams of "fixed air" (carbon dioxide) while melting 13 ounces of ice, and that these figures agreed fairly closely with those obtained when carbonaceous material was burned. At the same time he found that the animal consumed an amount of "eminently respirable air" or "air pur" (that is, oxygen) such that the respiratory quotient (carbon dioxide exhaled divided by oxygen consumed) was 0.84. Thus he made a long step toward the idea of the conservation of energy as well as the nature of animal heat; and the meaning of respiration was revealed.

Lavoisier freed chemistry and physics from the phlogiston theory; he showed the real nature of oxygen—to which he gave its name—and the part it plays in combustion. His crowning experiment was a complete determination of respiratory metabolism upon man with and without food and during various amounts of work, which he found involved a fourfold increase in the consumption of oxygen above that in the resting condition. His reflection on this is interesting, for he said:

> This kind of observations suggests a comparison of forces concerning which no other report exists. One can learn, for example, how many pounds of weight lifting correspond to the effort of one who reads aloud, or of a musician who plays a musical instrument. One might even value in mechanistic terms the work of a philosopher who thinks, the man of letters who writes, the musician who composes. These factors which have been considered purely moral have something of the physical and material which this report allows us to compare with the activities of the man who labors with his hands.

What he would have done had his life not been cut short may be gathered from the following passage, the conclusion of his last communication to the Academy:

> Up to the present time we have learned only to conjecture as to the cause of a great number of diseases and as to the meanings of their cure. . . . Before hazarding a theory we propose to multiply our observations, to investigate the phenomenon of digestion, and to analyze the blood both in health and disease. We will draw upon medical records and the light and experience of learned physicians who are our contemporaries, and it will be only when we are thus completely armed that we will dare attack a revered and antique colossus of prejudice and of error.

But the storm clouds of the French Revolution were gathering. Then as now, as always, after war and revolution, the scientific man who dares to think for himself is the inevitable victim of another type of man, the type of Marat, vulgar, aggressive, intolerant, the self-appointed agent of the new thing whether in government or in education. Lavoisier was sent to the guillotine. Doubtless always on great stages and on small, when the Marat type succeeds in grasping power for the short time that such men can be tolerated, their instinctive and first act will be to destroy those who guide their steps by a love of honor and of truth.

Were physiology merely biochemistry and biophysics, here I might stop. But there is yet another name which, although it came in the nineteenth century, stands out so pre-eminently as neither merely biochemist nor biophysicist but as a physiologist par excellence, that I must say in closing a few words about him—Claude Bernard.

CLAUDE BERNARD (1813–1878)
Reproduced from *The Popular Science Monthly*, 1878, 13, October.

With methods as simple for the most part as those available in the centuries preceding him, he revealed to us the vasomotor nervous system, the glycogenic function of the liver, the regulation of animal heat—the nature of carbon-monoxide asphyxia—to mention only a few of the greatest of his achievements. It is not, however, to these particular discoveries that I wish to point attention, but rather to a general idea which is in danger of being lost sight of nowadays in the enthusiasm for the solution of the problems of mechanism.

It is to Claude Bernard that we owe particularly the conception of regulation: the organism as a dynamic equilibrium, the organism as a whole, the organism in control of its environment. It is in such lines of thought, originating with Aristotle and coming down through Descartes, that physiology as physiology must be carried on by physiologists after biochemistry and biophysics have established themselves as sciences in their own right. Theirs are the problems of mechanism in the narrower sense, for physiology there remains the more general problem of living creatures as a whole and the coordination of their organs. It will be the service of biochemists and biophysicists to show, as Aristotle said, how "this and that part of a process (chemical or physical) is necessitated by this or that other stage of it." It is for physiology proper to study what Hippocrates called *physis*, and, as Aristotle also said, "to show that this and that process takes place for such and such a final object."

This is not vitalism. It is organicism. In biology a mechanist is like a mechanic whose attention is fixed on wheels and valves and bolts. A physiologist must not be like such a mechanic, but rather like an engineer who views the organism as a whole, both in the interplay of its parts, its integration, and in its exterior relations, the organism in its environment. This is a field in which we cannot perhaps expect for centuries still to come to attain wholly chemical and physical explanations. Three great examples in the recent development of such physiology occur to me: Pawlow's *Work of the Digestive Glands*, Sherrington's *Integrative Action of the Nervous System*, and Haldane's *Respiration*. Each is founded on the best physics and chemistry available, but each goes far beyond the physics and chemistry of the present day in showing correlation and regulation.

This conception of physiology was defined by Claude Bernard as a study of the body's capacity to "preserve constant the conditions of life in the internal environment." It is a conception which has been termed organicism. It prompts us to the most thorough analysis of which biochemistry and biophysics are capable into the problems of mechanism, but it looks on this analysis as a mere preliminary and sees as the essential topic of the physiologist those living reactions and processes by which (expanding Claude Bernard's statement) "the organism preserves constant" or rather adjusts, controls, and regulates within narrow limits of variation, such "conditions of life" as osmotic pressure, hydrogen ion concentration, temperature, content of sugar, calcium, and potassium, and a thousand other elements already known, suspected, or yet to be discovered "in the internal environment."

# SCIENCE ON THE MARCH IN THE NINETEENTH CENTURY

## THE REQUIREMENTS OF SCIENTIFIC EDUCATION*

By Prof. R. W. RAYMOND

THERE is danger that, in our newborn zeal for scientific education, we may sacrifice the interests of a truly liberal culture, producing, as I have said, a generation of specialists, incapable of appreciating the departments of human thought which lie outside their own, or even of rising within their own departments to broad and comprehensive views. We must not use the microscope till we spoil the eyes. We must not overtrain the investigator until he becomes less than a full man. The chemists, geologists, and engineers must not cease to be intelligent and active citizens. It may be demonstrated that such a mistaken neglect of studies outside the range of a chosen profession cripples activity and impairs success even in that profession. It is one result of the brotherhood of knowledge that no man, whether employed in the original investigation of Nature, or in the application of natural laws to practical ends, can advance successfully without perpetual communication of his thoughts to others, and the reception of their suggestions and experiences in return. Hence the mastery of language, which was the first condition of civilization, remains the essential condition of progress. The power to comprehend statements, logical arguments, and demonstrations, and to make such statements as may be comprehended by others, and will carry weight and influence in the very perfection of their form, is a vitally important part of the preparation of every young man for his life's career. His success, aside from his moral qualities, will be in direct proportion to his influence over other men; and this influence, again, will be in part proportional to his command of the means by which the minds of men are moved, mainly, language. Under this term we may include a knowledge of the methods of practical reasoning, and if this knowledge is best obtained by scholastic study of logic, then logic must be studied. If Latin and Greek are necessary, then they must be studied. For us, one thing is necessary—a thorough mastery of the English tongue—and this alone has been made to yield, in Lafayette College, a mental discipline not inferior to that of the classics.

But influence is not due to language alone. Behind this vehicle of thought there must be fullness and variety of thought itself. Those fruitful analogies, felicitous illustrations, graceful associations, which come, and come alone, through wide acquaintance with human life and literature, are so many elements of power, and, without this broad basis of a common ground from which to move the minds of others, the student of a special science, though possessed of the lever of Archimedes that would move the world, has no place whereon to stand.

In accordance with these principles, the object of the system of college education in America has always been development and discipline of character, and the broad preparation of the student for his subsequent special or professional pursuits. Our colleges may not have succeeded in realizing this ideal, nevertheless this has been their ideal; and it is the right one, as much today as ever. Whatever changes are required in our institutions of learning, to adapt them to the necessities of the modern era, must be changes in accordance with this principle —changes of means, not of ends, so far as colleges are concerned.

* Extract from the Inaugural Address at the dedication of Pardee Hall, of Lafayette College, Easton, Pa., by Rossiter W. Raymond, president of the American Institute of Mining Engineers.

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That changes are required is admitted on all hands. It is admitted that the physical sciences should be introduced to primary and preparatory schools; that they should be taught for the double purpose of mental discipline and of mental acquirement in the classrooms of our colleges; that in teaching them the scientific, inductive, experimental, instead of the dogmatic, method should be pursued; and, finally, that either connected with our colleges, or standing outside of them, schools of thorough scientific and technical special training are imperatively required. It is to inaugurate the wider activity of such a school that we are met here today, and I shall say a few words concerning the relation of this school to Lafayette College, on the one hand, and to technical education and the needs of the present time in technical departments on the other hand.

Although we trust that in time to come scientific investigation will be promoted in no mean degree by this school and its graduates, it must be confessed that at the present time its object is chiefly the preparation of young men for practical pursuits involving the applications of science. Nor can it be fairly said that this department is inferior in dignity to the pursuit of abstract science, so called. It is out of the ranks of the practical workers that those peculiarly gifted in scientific investigation are likely to arise; and it is in the ranks of practical workers that they must look, chiefly, for appreciation and support. It is no derogation from the value of a discovery of truth to say that it can be made useful to man; and, hence, there is no inferiority in the position of those who make it useful to man.

Indeed, that which the whole world chiefly needs today, and our country not less than any other, is the application of scientific truths and principles already known to the affairs, and labors, and problems, of daily life. We might even afford to pause in our career of fresh discoveries to consolidate the progress and utilize the results already obtained. But the alternative is not presented; it is not necessary or best that any part of the intellectual activity of the age should pause; the advance of science itself assists, and is assisted by, the applications of science.

We need a scientific in the place of a barbarous or scholastic architecture; a scientific in the place of a traditional agriculture; a scientific in the place of an empirical engineering; we need more machinery, more economical applications of power, more effective processes of metallurgy and manufacture, more exact knowledge, in all these particulars, of our own condition and necessities, and of the degree in which these can be supplied by experience already attained abroad. Lesoinne, a distinguished French writer, defines metallurgy as "the art of making money in the treatment of metals." This definition may be applied to almost all occupations of life. The practical art of each is not only to achieve certain results, but to do so profitably, to make money in doing so; that is to say, to increase the value of the raw materials, whether wood, or cotton, or ores, or time, or ideas, by the use we make of them, and the transformation to which we submit them, so as thereby to really elevate the condition of humanity: to leave the world better than we found it. This is, in its last analysis, the meaning of honestly making money. Men are put into this world with limited powers and with limited time to provide for their own sustenance and comfort, and to improve their condition. A certain portion of these powers and this time is required for the support of life in a greater or less degree of comfort, and with more or less multiplied means and avenues of enjoyment, activity, and influence. Whatever their labor produces more than this, is represented by wealth, and for purposes of exchange by money. To make money honestly, is to do something for other men better or cheaper than they can do it for themselves; to save time and labor for them; in a word, to elevate their condition. It is in this sense, greatly as we Americans are supposed to be devoted to making money, that we need to learn how to make more money; how to make our labor more fruitful; how to assail more successfully with our few hands the natural obstacles and the natural resources of a mighty continent; how to build up on the area of that continent a prosperous nation, united in varied, fruitful, and harmonious industries, glowing with patriotism and inspired by religion.

In this work we need especially the basis of a more thorough technical institution, applying principles of science to the material and economical problems involved. This education is necessary to supply the directing forces for the great agricultural, manufacturing, and engineering improvements of the country. It is also needed as a solvent and remedy for the antagonism between labor and capital. The true protection of labor will be found in its higher education, and in opening to the individual laborer for himself and for his children, by means of that education, a prospect of indefinite improvement and advancement.

In the realm of metallurgical and engineering operations the difference between theoretical and

practical training is, perhaps, still more striking. The student of chemistry in the laboratory cannot be made acquainted with many of the conditions which obtain in chemical and metallurgical operations upon a larger scale. All the chemists of the world failed to comprehend or to describe correctly the apparently simple reactions involved in the manufacture of pig iron, until, by the genius and enterprise of such men as Bell, Tanner, and Akerman, the blast furnace itself, in the conditions of actual practice, was penetrated and minutely studied. Moreover, in all the experimental inquiries of the laboratory the question of economy plays no part. It is the art of separating and combining substances which the student follows there, not the art of making money. That education of judgment and decision, of choice of means for ends which the exigencies of daily practice give, cannot be imparted in the school.

In mechanical engineering the same principle is illustrated. The highest department in this art is that of construction, and in this department the highest function is the designing of machinery. Now, the most perfect knowledge of the theory of a machine and its mathematical relations, of the strength of materials, or of the economical use of power, will not suffice to qualify a man to design a machine or a system of machines, for the reason that in this work an element must be considered not at all included in theoretical knowledge, namely, the element of economy in the manufacture, as well as in the operation of the machine. A machine, any part of which requires for its manufacture a tool (such, for instance, as a peculiar lathe) which is not already possessed by the manufacturer, and which, after the construction of this one part, would not be necessary or useful for other work—such a machine could not be profitably built. In other words, machines must be so designed, in a large majority of cases, as not to necessitate the construction of other machines to make them; and the planning of machinery, so that it shall be at once economical and durable in operation, and simple and cheap in construction, is not merely an important incidental duty, it is absolutely the chief and most difficult duty of the mechanical engineer.

## THE CINCHONA FORESTS OF SOUTH AMERICA*

By HENRY S. WELLCOME

IN THE month of June 1879, I visited some of the principal cinchona districts of South America. The following notes are based upon my own observation and information obtained from native bark dealers and gatherers. I shall speak more particularly of the cinchona forests of Ecuador, once the only source of bark, and still yielding large quantities. The bark territory is divided into the district known as Bosque de ("forest of") Guaranda and Bosque de Loja.

The Bosque de Guaranda is a vast forest, extending from about 1° north to 2° south; it covers with its rich verdure the western slopes of Chimborazo, and the outlying ranges of the Cordilleras to more than ten thousand feet above the sea level, encompassing within its higher limits the picturesque city of Guaranda. This district is the source of most of the barks exported from Guayaquil and has never yet been fully explored. Guayaquil, the main shipping port of Ecuador, is a city of thirty thousand inhabitants, situated on the Guayaquil River, sixty miles from its mouth. The river is navigable to this point by large ocean steamers. The southern extremity of the Bosque de Guaranda is reached at Pueblo Nuevo, a small town on one of the eastern branches of the Guayaquil River, about seventy-five miles from this town. A small steam launch plies between the places.

The elder cinchona district, Bosque de Loja, was the source of the first barks taken to Europe. It extends from 2° to 5° south, to the boundary line of Peru, and covers the western slope of the Cordilleras below the timber line. This district has been worked constantly for over two hundred years, and the quantity of bark it furnishes to the Guayaquil market has fallen off in recent years.

At Pueblo Nuevo, mules and servants were engaged for the journey to the mountains. Wheeled vehicles are useless, for want of roads, and all

* Reprinted from *The Popular Science Monthly*, 1880, 17, 507.

transportation is done on the backs of beasts or Indians. Before reaching the highlands, forests of ivory-nut palms, with their long, graceful, feather-like branches, and scattered trees of *Cinchona magnifolia,* a valueless species, are met with. Occasionally we found clearings, with extensive *haciendas* of cacao, coffee, sugar cane, and anatto. The farther we got into the higher mountains, the more the difficulties and dangers increased, and at last a point was reached where the mules had to be abandoned, and, after continually ascending and descending steep places, a point on one of the great spurs was reached, whence was seen an undulating sea of wilderness as far as the eye could reach—a gorgeous expanse of matted verdure; here and there tall, slender columns of gigantic palms pierced the forest roof, and gracefully waved aloft their drooping branches and leaves; and now and then a huge bank of clouds drifted up, like a Newfoundland fog, curtaining the grand scene for a few moments, and then quickly passing off. Our *cascarillero* soon descried some cinchonas in the distance by their glistening leaves, which reflected brightly the vertical rays of the sun.

This characteristic reflex of the foliage, with the bright, roseate tints of the flowers, and in some species also of the leaves, affords the means of discovering the cinchonas among the mass of the forest giants. The glossy leaf of the India rubber tree is easily mistaken for the cinchona, but skilled *cascarilleros* are usually able to distinguish, at a great distance, varieties by the color of the flowers and general appearance of the tree.

At the bottom of a ravine we followed a small stream, till suddenly our guide shouted, *"Cascarilla!"* and we were gladdened by the sight of a number of fair-sized trees of *Cinchona succirubra.*

The cinchonas seek the most secluded and inaccessible depths of the forests; they are rarely grouped in large numbers or close together, but are distributed in more or less irregular, scattering patches. The older trees are grand and handsome, forty to eighty feet in height, trunks straight, branches regular, leaves evergreen (six to ten inches long), of a dark-green color, sometimes tinged with crimson, the upper surface of an almost waxy luster, flowers in terminal panicles of bright rose-tint and of an agreeable fragrance. The bark of the large trees is usually completely covered with mosses of the most delicate, lacelike texture, interspersed with lustrous, variegated lichens and diminutive, trailing ferns, while air plants and vines in profusion entwine themselves among the branches of the trees, and hang in graceful festoons, forming hammocks in which clusters an abundance of parasitic growth, particularly of the orchid family. Vegetable growths develop with wonderful luxuriance beneath the interlacing branches, which permit but little access of sunlight underneath. Everything is saturated and dripping with moisture; the very air we breathed seemed a clammy, aromatic vapor. In these vast forests atmospheric changes are continuous and abrupt; drifting banks of gloomy clouds are followed by glaring sunshine, and then tempestuous showers—all in rapid succession. The temperature is more even, averaging about 65° Fahr., seldom exceeding 80° or falling below 45°, the altitude being about six thousand feet.

To discover cinchona tree patches in the forests, the *cascarilleros* ascend such high spurs as command a good view of the surrounding valleys and mountain slopes. After discovering a forest that indicates sufficient value to render it profitable to work, a certain limit of forest land is condemned and a claim made to the government; upon the payment of a certain fee, a title is granted on very much the same plan as those upon mining claims in the United States. The next step of the *cascarillero* is to apply to a bark-dealer for funds with which to work the claim: if he can present satisfactory evidence that his forest is a profitable one, sufficient money is usually advanced, the merchant holding the title as security, with an agreement that the bark shall be delivered and sold to him exclusively; sometimes the dealers purchase claims outright, and employ men to work them. For many years the bark trade of Bolivia was monopolized by the government; the *cascarilleros* were obliged to sell their bark to a bank established for the purpose, and receive for it whatever price the officials chose to pay. This system was conducted with such flagrant injustice and dishonesty that it was finally broken up. Now each republic levies a duty on all barks exported.

The season for bark gathering begins about the first of August (in some forests as early as June), and lasts till October or November; during these months the bark cleaves most readily, and, on account of less rainfall, the forest is more accessible. It is next to impossible to enter it during the wet season. A master *cascarillero* with his gang (sometimes several hundred peons) establishes a main camp in the forest, on an elevated point where there is an opening in the forest, so as to allow the bark as much exposure as possible. The peons are formed into squads, and scatter through the forest, establishing small camps. When they are ready for work, and the bark gathering begins, one or two from each division seek out the trees, while others

cut down and peel them. The trees are first decorticated from the ground up as far as can be reached, and then, after felling and removing the clinging vines and mosses, the rough, outer bark is beaten off with a club or mallet. The bark is then cut around the trunk in sections of two to three feet, and longitudinally in strips of six to eight inches in width, then removed with the blade of a *machete*.

When first taken from the tree the inner surface of cinchona bark shows a handsome cream tint, but, on exposure to the atmosphere, rapidly darkens to a dirty red. The barks are usually taken to the main camp for drying and storage. The thick bark of the trunk requires great care in drying, because of the excessive dampness of the atmosphere, which sometimes necessitates the use of artificial heat to prevent molding; it is piled up in tiers with sticks between the layers to allow free circulation of air, and heavy stones or fragments of rocks are placed on top to flatten it. The thin bark from the young trees and small limbs dries more rapidly, and rolls itself up into quills.

One of the greatest difficulties connected so far with the gathering of cinchona bark is that of transporting it to the coast at the end of the season. It is roughly sorted, mainly according to the part of the tree from which it is obtained, and packed in bales of about one hundred and fifty pounds each; the Indians carry these bales on their backs a distance of sometimes hundreds of miles to a transfer warehouse, whence it can be transported by mules to the nearest shipping place. The worn appearance of most flat bark of commerce is due to the long friction which it undergoes during transportation.

The Indians, in carrying bark, bear the main weight of the burden upon their heads, by placing over the forehead a strip of rawhide to which are attached cords of the same material lashed to the bale; they stoop forward to maintain their equilibrium, and use long Alpine sticks to steady and aid them in ascending or descending dangerous cliffs. The skeletons of hundreds of wretched peons can be seen in the far depths of the chasms below some of the older trails, bleaching beneath the tropical sun, their earthly toils ended by a misstep on the verge of one or the other frightful precipice; now and then ghastly human skulls are seen placed in niches or crevices in the projecting rocks of the mountainsides along the narrow passage, suggestive of lurking dangers. Another fearful terror to the Indians is the malarial fevers, to which they quickly yield, owing to great exposure and want of nutritious food. It was said that, during a recent severe malarial season, as many as 25 per cent of the Indians employed in one district died from fevers before the harvest was completed, and it is only by extreme poverty, or obligations as peons, that they are induced to enter the bark forests to encounter the dangers for the meager pittance of ten to twenty-five cents per day.

The final sorting and classifying of bark are done at the main storehouses at the coast, where it is closely packed in ceroons of previously moistened cowhides (hair-side out), or in bales of heavy sacking. There it is that most of the adulteration is done. The admixture of inferior barks with higher grades is not so much the result of ignorance as sometimes supposed, for the bark-dealers are generally very expert in determining the different varieties and estimating the values of barks. Yet, strange to say, very few bark-merchants ever become wealthy.

All barks enter the market bearing certain brands; these brands used to gain a reputation according to the quality of bark they represent, but frequent occurrence of sophistication of reputed brands with inferior grades of bark has brought on the result that large buyers do not any more purchase cinchona barks without first making careful assays, but even with this precaution they are sometimes deceived, on account of the adroit manner in which the barks are mixed.

The points of shipment for Ecuadorian barks are Guayaquil and Esmeraldes; for the barks of northern Peru, Payta; from southern Peru and Bolivia, Arica, Islay, Iquiqui, and Callão. A limited quantity of Bolivian bark is exported by way of the Amazon to Pará. The greater portion of the bark produced in the northern and eastern districts of the United States of Colombia reaches the market by way of Carthagena and Baranquilla on the Caribbean coast, but that collected in the state of New Granada is mostly shipped from Buenaventura on the Pacific coast. Venezuela furnishes very little bark, and that is sent from Puerto Cabello.

As regards the prospects for future supplies of cinchona barks from the native forests of South America, the outlook is exceedingly discouraging; the greatly increased use of cinchona alkaloids during the last few years, with the consequent demand for larger supplies of bark, has caused a very thorough working of the old forests, and energetic seeking for new ones. The discoveries of paying forests are becoming more and more rare every year, and the new forests are found at greater distances from the shipping ports, and are more difficult of access.

The tract of country yielding the cinchona is not so unlimited as some writers would lead us to believe, nor is the supply inexhaustible; it is a fact recognized by natives and dealers, who are well informed about the extent and resources of the cinchona-bearing districts, that if the present ruinous system of destroying the trees is continued, and no effort made to propagate new growths, they will, before many years, be practically exterminated from their native soil.

With the abundance of seeds yielded by the cinchonas, one would naturally expect young plants to spring up in great numbers, but such is not the case; the light-winged seeds mostly fall upon and adhere to the ever-moist foliage, where they quickly germinate and decay; or, if perchance they fall to the ground, it is exceedingly difficult to gain a rooting, as the soil is covered to a depth of ten to twenty inches with loose, decaying leaves. Beyond all doubt, the cinchonas might be successfully cultivated in their native country, especially in the exhausted forests; but the natives show no enterprise, and foreigners receive no encouragement from the governments to attempt it. . . .

The almost continuous revolutions and wars in those South American countries so unsettle everything as to render investments hazardous; the roads and ports are sometimes blockaded for months, preventing the importation of goods or shipment of barks, often entailing heavy losses upon the dealers.

In case of war or revolution, every Indian peon is subject to military duty, and, if required, is forced to enter the army; sometimes it is impossible to obtain sufficient *cascarilleros* to make it pay to enter the forests; hence it is that political troubles in those countries so greatly influence the price of bark and quinine.

The efforts of the Dutch and British governments in taking energetic and extensive measures, by establishing vast plantations of cinchona trees in their eastern colonies, to insure against the possibility of the world's bark supply becoming exhausted, are therefore of paramount importance; and it is a matter of general concern and gratification that their experiments are proving from year to year more successful, . . .

## WOOD'S DISCOVERIES AT EPHESUS*

EPHESUS, one of the twelve Ionian cities of Asia Minor, was famous in antiquity as containing one of the seven wonders of the world, the great Temple of Artemis, or Diana. From very early times Ephesus was a sacred city; the fable ascribed its foundation to the Amazons, and the Amazonian legend is connected with Artemis. The first Ionian colonists in Lydia found the worship of the goddess already established here in a primitive temple, which was soon superseded by a magnificent structure. This Grecian temple was seven times restored, at the expense of all the Greek communities in Asia Minor. In the year 365 B.C. it was burned to the ground, but again rebuilt in a style of far greater splendor than before, the work extending over two hundred years. This later temple was 425 feet long and 220 feet wide. "The foundations were sunk deep in marshy ground, as a precaution against earthquake," says Pliny. There were two rows of columns at the sides, but the front and back porticoes consisted of eight rows of columns, placed four deep. Outside, at the entrance to the temple, stood a basin of porphyry, 15 feet in diameter, for the worshippers to lave and purify themselves in. The internal decoration was of the most sumptuous kind. The cedar roof was supported on pillars of jasper; the doors were of cypress. The altar was the work of Praxiteles, and it was surrounded by many statues, one of them of gold. The image of the goddess herself was roughly hewed out of wood, black with age, and greasy with the oil with which it was customary to anoint it. When the apostle Paul visited Ephesus in the middle of the first century, the worship of Diana still flourished there, and the temple retained all its original splendor. . . . The Goths sacked the city and burned the temple, about two hundred years later, and in the reign of Theodosius I, toward the end of the fourth century, the furious zeal of the Iconoclasts, or Image-breakers, completed the work of destruction. The ancient city almost entirely disappeared before the modern era, the very site of the temple being lost.

In 1863 an Englishman, Mr. J. T. Wood, while engaged as a civil engineer in constructing a rail-

* Abridged from *The Popular Science Monthly*, 1875, 7, 223.

way from Smyrna to Aidin, discovered at Ayasalouk the ruins of the Odeum, or Lyric Theatre of Ephesus, and this circumstance led him to commence excavations in that locality in search of the Temple of Diana. He began his excavations on the west side of the ancient city, at a point where a long rise of ground above the level of the plain seemed to cover the portico of the temple. Here he found nothing but the remains of a Roman monument; so he went on digging trail holes in every direction on the west side, and explored the great Gymnasium, which proved to be a Roman building, erected on the site of a former Grecian structure of similar character. On the surface of the ground, in the vicinity of this Gymnasium, were the remains of some columns of Egyptian silex. . . .

But there was yet no sign of the temple, and the literary remains of antiquity gave no indication as to its site. His private funds being now exhausted, the trustees of the British Museum were applied to by Mr. Wood for the means necessary to carry on the work of exploring the Odeum, or Lyric Theatre, in the hope of finding there some bas-relief, or other monument, or at least some idle scratching of a rough artist of the time, which might give some indication of the site of the great temple. In this hope he was encouraged by what he had years before seen in Venice and other places, viz., the plans of cities cut in bas-relief upon the pinnacles of the churches. The trustees of the British Museum having made the required grant of funds, Mr. Wood began the exploration. . . . of the great theatre in February 1866. This was one of the largest structures of its kind in Asia Minor, being 495 feet in diameter, and capable of seating 24,500 persons. Here were found many interesting Greek, and a few Latin inscriptions—chiefly decrees of the senate and people of Rome. . . .

In January 1868, he put a gang of seventy men to work at the great theatre, and at the same time began to follow up the road leading from the Magnesian Gate. This consisted of three openings —two for foot passengers, and one for wagons and chariots. The pavement was intact, with four distinct chariot ruts cut into it. Having followed up this road for about 700 feet, Mr. Wood came upon the stone piers of a portico 12 feet wide. This was undoubtedly the grand portico built by Damianus, a rich Roman noble. Many tombs were found, some of which were vaulted chambers finished in stucco or cement, and painted, . . . Next he hit upon a corner of the Peribolus Wall, on which were inscriptions showing that this wall was built in the time of Augustus.

This was in May 1869. The discovery of the Peribolus Wall proved sufficient to induce the trustees of the British Museum to make further advances of money, and accordingly work was resumed in the October following. In the area within this wall, i.e., in the sacred precinct of the temple, Mr. Wood sank a great number of trial holes. Nothing of interest was discovered until the explorer had proceeded about half a mile from the angle first discovered, and then remains of Roman buildings began to be found. Soon he came to a long line of Roman buildings which must have been the dwellings of the priests and priestesses of Diana. . . . Finally in the very last day of the year 1869 he hit upon the pavement of the temple itself, more than twenty feet underground. . . . The pavement was all beautiful marble. It was in two layers: the upper course in white marble, the lower one in cement, making altogether a thickness of two feet.

The discoveries on the site of the temple in the season of 1872–73 comprised two large fragments of the frieze with human figures, life-size, in high relief, and the figure of a stag; the base of one of the inner columns of the peristyle; two sculptured drums of columns; some lions' heads, from the tympanum at the west end of the temple; a large fragment of a cedar beam from the roof, and a number of fragments from the last three temples. . . .

Work was suspended in May 1873 and resumed in October. During the season of 1873–74, Mr. Wood made discoveries which enabled him to complete his plan of the temple. More than 100 feet of the lowest steps of the platform were found in position in different parts; also a sculptured drum, with draped figures alternately seated and erect. At the beginning of 1874, Mr. Wood, having only a small balance on hand, applied to the trustees of the British Museum. He was allowed only a small sum, with instructions to close the work when it was expended. He therefore began to remove the cella walls, and found distinct remains of the last three temples. Part of the pavement of the temple destroyed by Erostratus was found in position, and also the altar at the east end of the cella, or shrine, which must have served for the three temples; also about 200 fragments of sculpture and architectural enrichment, of which the piers had been composed. . . .

On extending the excavation 30 feet beyond the lowest step of the platform, a wide portico was found, which must have surrounded the temple on three sides, and also the remains of a Grecian Doric building, which could not be explored for want of funds. . . .

*(J. T. Wood was a Fellow of the Royal Institute of British Architects.)*

# LITERARY NOTICES

POPULAR LECTURES ON SCIENTIFIC SUBJECTS. By H. HELMHOLTZ, Professor of Physics in the University of Berlin. Translated by E. ATKINSON, Ph.D., F.C.S., Professor of Experimental Science, Hoff College. With an Introduction by Prof. Tyndall. New York: D. Appleton & Co., 1873.

THIS is, in several respects, the most important scientific publication of the season, its interest being due not alone to its valuable contents, but quite as much to the form in which they have been put by their illustrious author. When the present MONTHLY was started, surprise was expressed in various quarters at the broad scope of its discussions, which it was said went far beyond the legitimate meaning of our title. Science being considered as a kind of tough and forbidding knowledge belonging to laboratories, observatories, and apothecaries' shops, popular science was regarded as the same kind of knowledge loosely stated in common language. At the outset we rejected this view as narrow and false, holding that science, instead of pertaining to certain things, consists in a method of knowing, which applies to all things that can be known, and that popular science must be equally comprehensive. Science itself being progressive, its great army of workers is constantly engaged in extending and correcting it by numberless processes of original investigation, while it is the office of popular science to bring its conclusions, applications, and results into the sphere of common thought. Learned men long neglected the duty they owed to the public to clothe the result of their labors in authorized and acceptable forms for general use, and the consequence was that this work was done by incompetent hands, and degenerated into mere amusement and recreation; but, with the progress of liberal opinion, the diffusion of education, and increasing respect for the rights and welfare of the people, eminent men of science have turned their attention seriously to the task of embodying their ideas in popular form.

In his introduction to the present volume, Prof. Tyndall remarks: "One evening during my residence in Berlin, my friend Dr. Du Bois-Reymond put a pamphlet in my hands, remarking that it was the 'production of the first head in Europe since the death of Jacobi,' and that 'it ought to be translated into English.'" That "first head in Europe" was on the shoulders of Helmholtz, and the pamphlet was his celebrated essay on the "Interaction of the Natural Forces," which has been extensively circulated in this country, and is one of the most elegant and popular expositions of the doctrine of the "Conservation of Force" that has appeared in any language. The first complete work of Prof. Helmholtz in English is the volume now issued, consisting of popular lectures on scientific subjects. Speaking of these lectures in his preface, the author says: "If I may claim that they have any leading thought, it would be that I have endeavored to illustrate the essence and the import of natural laws and their relation to the mental activity of man. This seems to me the chief interest and the chief need in lectures before a public whose education has been mainly literary." It is gratifying to note that this statement of the chief aim of popular science entirely coincides with the view presented in the prospectus of THE POPULAR SCIENCE MONTHLY. It is not the illiterate that are to be addressed, but the classes that have received such cultivation as the prevailing educational system affords, while the development and illustration of natural laws in their bearing upon the higher nature and elements of man is [sic] the ultimate and most important end to be attained.

Hermann Ludwig Ferdinand Helmholtz was born at Potsdam in 1821. He studied medicine, and was at first military physician and afterward assistant at the Astronomical Museum at Berlin in 1848. From 1849 to 1852 he was Professor of Physiology in the University of Königsberg. He became Professor of Physiology at the University of Bonn in 1855, and in 1858 accepted the physiological chair in the University of Heidelberg. He is now reëstablished in Berlin as Professor in the university of that city. Prof. Helmholtz has attained a recognized preëminence in three great departments of knowledge—physiology, physics, and mathematics. He began with the study of physiology, but, finding that to be dependent upon physics, he proceeded to master the physical field. But here, finding again that physics depends upon mathematics, he pushed on to the conquest of this department of science. His great works are on "Physiological Optics" and "The Physiology of Audition," and by his thorough acquaintance with physics and mathematics, he has greatly enriched and extended our knowledge of the science of these higher senses. Prof. Helmholtz's intellect is characterized by great breadth and synthetic grasp, which leads him to take large views, and treat the subjects he enters upon with comprehensiveness. The opening and closing papers of the present volume—the first, "On the Relation of Natural Science to Science in General," and the last, "On the Aim and Progress of Physical Science"—are admirable examples of this broad judicial treatment of the subjects discussed. His statement of the reactions of science and philosophy in Germany, and the influence of the German universities upon contemporary thought, in the first paper, is especially admirable. The volume also contains very able articles upon his special subjects of investigation—one "On the Physiological Causes of Harmony in Music," and another elaborate paper, in three parts, "On the Recent Progress of the Theory of Vision." There is also a very interesting lecture "On Goethe's Scientific Researches," and an elaborate dis-

cussion of glacial phenomena. Two papers are also given "On the Interaction and Conservation of Forces," a subject which Prof. Helmholtz has pursued independently, and which in these expositions is presented in its fundamental principles. Numerous illustrations enhance the instructiveness of the volume, which, though compactly written, is still remarkably clear in its explanations. Prof. Helmholtz is an eminent master of the art of statement, but, as his thoughts appear in a foreign language, the force and finish of the original composition are not to be looked for. Yet the several translations of this volume by Professors Eve, Ellis, Atkinson, Tyndall, and Drs. Flight and Pye-Smith, have been made with great care, so that the work is as attractive and readable in style as it is solid and instructive in its thought. We commend this book to all who are interested in the higher scientific problems of the age, as treated by one of its masterminds.—*The Popular Science Monthly,* 1873, **3**, 513.

ANNUAL RECORD OF SCIENCE AND INDUSTRY FOR 1873. Edited by SPENCER J. BAIRD. New York: Harper & Bros. 714 pp., 12mo. Price, $2.00.

THIS volume presents a very large amount of valuable and interesting information in compact form, being in fact a history of progress in science and art for the year. The main part of the book is prefaced with a brief summary of the year's progress. The matter is presented in divisions named according to their nature, in general scientific terms, as Mathematics and Astronomy, Terrestrial Physics, and Meteorology, etc. Much important knowledge bearing directly on the ordinary affairs of life is to be gleaned from the divisions on Agriculture and Household Economy. For instance, the latter contains some valuable facts about lightning and lightning conductors. Chimneys should be kept clean, as one lined with a thick layer of soot is dangerous, being apt to conduct the current of electricity into the house. The costly copper rods now so popular are condemned, and the ordinary galvanized iron wire, No. 4, recommended instead. A conductor, to be effective, should have no joints nor acute angles, and the lower end should rest in the ground, while the upper should be tipped with a gilded or polished point. Conductors are also likely to become impaired from use, and therefore need occasional examination and repairing. In the division of Materia Medica, a simple and effectual method is given for distinguishing real from apparent death. This is in simply tying a tight ligature around a finger of the supposed corpse; if death is only apparent, the end of the finger will shortly become red.

About meteors and comets, we are told, Prof. Proctor has concluded that comets are detached masses of matter thrown off by planets like Jupiter, Neptune, and Uranus, while in a molten condition. Meteors are fragmentary parts of disintegrated comets. The inflammatory character of meteors has also been established. In May, 1873, two men in North Germany observed a falling meteor strike against a church-tower, and rebound with loud detonation to a housetop. The house soon became enveloped in flames, which spread and destroyed several adjoining buildings. The most startling statement, probably, comes from Secchi, the great Italian astronomer. This is that the sun varies in size. Secchi's hypothesis is that the sun's photosphere as seen by us is a gaseous envelope, continually, and perhaps periodically, changing in apparent size.—*The Popular Science Monthly,* 1874, **5**, 249.

THE GREAT ICE AGE AND ITS RELATION TO THE ANTIQUITY OF MAN. By JAMES GEIKIE, F.R.S.E., etc. New York: D. Appleton & Co. 8vo., 525 pp. Price, $2.50.

THERE are many persons who would listen with an air of scornful incredulity to the statement that the hills and valleys of New York and New England, which summer now clothes with a mantle of luxuriant verdure, were once a dreary, desolate waste, covered up by a crust of ice many hundred feet thick. Nevertheless, the fact is indisputable; the mountains, the rocks, the configuration of the soil, even the fragmentary stones that lie upon the surface, point silently, eloquently, and immovably to the fact. Geologists had long noticed, in valleys adjoining mountainous districts, certain long, low ridges, called "sowbacks," running parallel to each other and trending down the valley. They had dug into these ridges and picked out flat, oblong stones, with strange scratches upon their surfaces. They had noticed that, while the mountainsides looked jagged and rugged from below, from above they presented a rounded and undulating outline to the very base of the mountain. It was also noticed that the rocks on the mountainsides displayed on their undulating or upper surface the same mysterious scratches or striae that were observed on the stones embedded in the ridges below. All of these signs greatly puzzled the geologists, and various theories were invented to account for them, but their true significance was not dreamed of until the late Prof. Agassiz, from the study of Alpine geology, announced that they were the results of one and the same cause—glacial action; that is, that the whole face of the country was covered to the depth of two or three thousand feet with solid ice, which, in gradually creeping toward the ocean to shed its bergs, had worn the mountainsides into waves; broken, scratched, and transported the rock to distant points, and furrowed up the soil of the valleys through which it continued to crawl seaward. Unmistakable evidence existed that this arctic condition of climate prevailed all over Europe, Asia, and America, northward of 45° north latitude; that is to say, that the vast area comprehended within that circle was once covered with ice as completely as parts of Greenland and the rest of the country immediately around the North Pole is [*sic*] now covered. Of course no life could exist under such conditions, and it was therefore supposed that the advent of man, within that circle at least, must have occurred subsequent to their passing away. It is on this point that Mr. Geikie's book throws a flood of light. He describes the evidences of the glacial condition with admirable skill and clearness, and then proceeds to

consider its bearing on the antiquity of man. The earth and stones, or "rubbish," that covers the rocky foundation of the countries comprehended in the circle described, is called the "Drift or Glacial Formation," to indicate that it was deposited thereon by glacial action. The drift is divided into two parts, the upper drift and the lower drift. The lower is, of course, the oldest formation. It is composed of a "tough, stony clay," colored like the rocks about which it lies, and small, fragmentary stones, flattened and scratched. The mass of clay and stones is called "till." The till is not laminated, but pressed down in a confused mass, and its coloring shows it to have been produced by comminution of the rock upon which it lies, while the rock itself corroborates that testimony by being scratched and polished like the stones in the till. Thus the till was formed by the grinding of the ice against the rock. Deeply embedded within the till, occur at intervals deposits of sand and gravel, such as we find at the bottom of lakes and rivers. But how could there be lakes and rivers to deposit sediment, while the whole country was covered by a crust of ice more than a thousand feet thick? This is a question that has long puzzled geologists. But only because the true significance of these sand and gravel deposits was not before seen. Mr. Geikie has pointed out that the deposits occurred during an intermission of the Great Ice Age, when the ice melted and disappeared from the land, which became clothed, instead, with trees and plants, and peopled with animal forms. In the course of ages the arctic conditions returned and covered the land again with ice. He has also pointed out that this alternation of temperate and arctic climate has certainly occurred more than once, probably several times. Mr. Geikie's inference becomes still stronger when viewed in the light of Mr. Croll's new theory of a periodic change of climate resulting from the precession of the equinoxes and the increase of eccentricity in the earth's orbit. We have no room for an explanation of Mr. Croll's theory, but must content ourselves with referring the reader to Mr. Geikie's book, where he will find it lucidly stated.

Now, these facts have a very important bearing on the history of man. The remains that we have gathered of primitive man are divided into *paleolithic,* or those belonging to the Old Stone Period, and *neolithic,* or those belonging to the New Stone Period. The *paleolithic* remains are characterized by the rudest kind of stone implements, implements merely chipped out of stone, without any attempt at finish, and from first to last there is no evidence of improvement in their make. The *neolithic* implements, on the other hand, are much better made at the starting point, and they gradually improve, until they give place to implements of bronze. Again, the *paleolithic* remains are accompanied with the remains of mammalia, such as the mammoth, etc., which are now wholly or locally extinct, while the mammalian remains found with those of *neolithic* man are of existing species. Lastly, the *paleolithic* remains are found in the deposits of sand and gravel we have described as imbedded within the till, while the *neolithic* remains are found only in the upper drift. Thus in one and the same way the existence of man is shown to extend to interglacial, probably to preglacial times, and the meaning of the apparent gap in his history between the *paleolithic* and *neolithic* ages is explained. The *paleolithic,* or interglacial, perhaps preglacial man, was driven from the country, or destroyed by the change from a mild to an arctic climate; and, when the arctic conditions passed away for the last time, his place was filled by the *neolithic,* or postglacial, man, from more southern latitudes. We have indicated in a necessarily general manner the central idea of Mr. Geikie's book; it contains a great deal of very interesting information of a subordinate character, which will amply repay perusal.—*The Popular Science Monthly,* 1874, **5**, 377.

CONTRIBUTIONS TO SOLAR PHYSICS. By J. NORMAN LOCKYER, F.R.S. New York: Macmillan & Co. 1874. 676 pages. Price, $10.

THE avidity of the general public for information in regard to recent researches in physics, and particularly in regard to researches made by aid of the spectroscope, is witnessed by the number of volumes which have appeared within the past few years devoted solely to the popular exposition of these subjects. The announcement of a new book with the same purpose is one which, we should fancy, the average reader of these books would receive with mingled feelings. It seems to us that this average reader, while feeling that it was his duty to rejoice that the class which he represents was being so very fully supplied with treatises on a certain class of topics, would likewise begin to doubt whether the topics themselves had not been exhausted.

At least he might doubt whether the popular exposition of them had not been carried to an extreme point. Certainly it seems very hard to add to the books of Roscoe and Schellen anything in regard to the fundamentals of Spectrum Analysis which shall be worth adding. It is easy to conceive our average reader turning the pages of a new book of this sort with a kind of nervous fear, lest he should come across those tiresome woodcuts of a German-looking man gazing intently into a prism in the hope of seeing a candle flame double, or of two sombre individuals shut up in a dark and very large room, alone with Newton's experiment. These woodcuts he has seen for years, and they seem to him as the brownstone houses on the Fifth Avenue seem to the weary traveler; milestones that he can never pass—"a procession which never gets past its given point."

Now, we distinctly sympathize with our average reader and we claim that a book of this nature, to be necessary or even acceptable at this time, must be a decided step in advance of the former ones.

The volume before us contains 676 pages (including a good Index), and it is divided into two parts: Part I is devoted to a popular account of ancient and modern sun work; Part II contains communications made by the author to the Royal Society of London and to the French Academy of Sciences. Added to these we have sixteen valuable Notes on various special points; and two Appendices, one giving the "Instructions to Observers

of the Eclipse of 1871," and the other being Respighi's "Memoir on Solar Prominences."

To consider the volume in inverse order, we may say that, of Part II, the valuable Notes and Appendices are the only parts which ought to have been given in their present form, according to our judgment, and it may even be doubted whether the Notes should not have been worked into the text. Mr. Lockyer says, in regard to the contributions to the Royal Society and to the Academy of Sciences, that they are "of course" given *verbatim*. Here the author has, it seems to us, forgotten the proper object of his book. These papers were written from time to time as results began to come from the "new method," and they are of necessity incomplete. The only object in giving them in their original form is to show exactly Mr. Lockyer's relation to the progress of discovery and research. But the history of the subject is very well known among all interested in it, and Mr. Lockyer's foremost place in certain branches of it is too well established to need a repetition of the formal proofs.

To the scientific worker these papers are already accessible in the original, and, for general purposes, they should have been entirely recast, as they are decidedly not in the best form now.

With regard to the first part of the work we may say that much of it is a repetition of matter which has been thoroughly treated in other books. Some of it consists of accounts of eclipse work which the author himself did, and the story of this is told in a thoroughly good and interesting way. Mr. Lockyer's accounts of the work of others are eminently fair and exhibit good feeling and entire appreciation. The chapter on Mr. Carrington's researches on solar spots is an example, and the author's account of the discovery of the new method of viewing prominences which was applied by Janssen and himself, though first conceived by Lockyer, is thoroughly admirable for fairness and candor.

Part I, however, has more serious defects than the final section of the book. It is entirely deficient in judicious arrangement, and its perusal can only confuse the ideas of the learner. It is, in fact, a reprint of essays (each of them good in itself and in its place) which Mr. Lockyer, sometimes alone, sometimes in concert with Mr. Balfour Stewart, contributed to English periodicals, and of occasional lectures.

It abounds in repetitions; sometimes whole paragraphs, almost pages, are printed at least twice, and the whole seems to show a desire, to speak plainly, to "make a book."

We must insist that, while it is, abstractly, a thing to be grateful for that Mr. Lockyer should give his valuable time to the popular exposition of scientific truths, some of which he has been so fortunate as to discover, it is, in the case before us, still a fact that he has added scarcely anything to the ample information in regard to them which is now accessible, and nothing at all to his scientific reputation.

His character as a man seems to be shown, in his account of his relation to other scientific men—his friends—and that is almost the only outcome of this expensive volume, whose principal fault is a want of a sufficient *raison d'être.*—*The Popular Science Monthly,* 1874, **5**, 120.

My Visit to the Sun; or, Critical Essays on Physics, Metaphysics, and Ethics. By Lawrence S. Benson, author of "Benson's Geometry." New York: James S. Burnton. 8vo., 157 pp. Price, $1.50.

IF WE were called upon to state the object for which this book was written, we should say that it was to display what the author evidently fancies to be a very wide and accurate knowledge of science. With all the flourish and clatter of a Don Quixote charging the windmill, he impinges the mighty lance of querulousness against the feeble form of gravitation, utterly annihilating that venerable body. The atomic theory in chemistry, and the Fayian and Franklinian theories in electricity share the same fate, as do many kindred absurdities long fostered by the ignorance of man. And, as if those blows did not inflict punishment enough on the physicists, they are utterly crushed by the entirely new and astonishing revelation that final causes are unknowable. The present volume is on physics, and the most appalling fact that it contains is the announcement that it is to be followed by similar volumes on metaphysics, ethics, etc.—*The Popular Science Monthly,* 1874, **5**, 378.

Our Common Insects. A Popular Account of the Insects of our Fields, Forests, Gardens, and Houses. Illustrated with 4 plates and 268 Woodcuts. By A. S. Packard, M.D. 225 pages. Price, $2.50. Salem: Naturalists' Agency. Boston: Estes & Lauriat. New York: Dodd & Mead.

DR. PACKARD has done an excellent thing in preparing this little hand-book. His large "Guide to the Study of Insects," with upward of 700 pages and 1,200 figures, although reduced to five dollars in price, is still too expensive for the great mass of readers; and it was therefore well to distill it over, with the contents of the *American Naturalist,* into a more portable and popular form. Good and cheap books on insects require to be multiplied, for we are all interested in them. They infest us inside and out, by day and by night, sleeping and waking, at home and abroad; they damage our food, poison our drink, spoil our clothes, kill our domestic animals, ravage our gardens, blast our fruit, and destroy our crops. The subject cannot be ignored, but we naturally approach it with prejudice. There are, however, compensations in all things. Although insects may be our enemies, they are yet scientifically very interesting creatures. We all have a high opinion of Nature, and are never done praising her; but she runs to insects incontinently—they could outvote all the rest of the animal kingdom five to one. As the higher tribes of life have been perishing out in multitudes along the geological march, it cannot be doubted that the same thing has happened in a much greater degree to the insects,

although their vestiges were, of course, more difficult of preservation. But Dr. Packard tells us that there are upward of 200,000 living species, and, as species are held by many to be immutable, each one having been specially created, we have a clew to the exact number of miracles that these pests have cost: though why miraculous contrivance took such an excessive turn in this direction will perhaps be found explained in Dr. Bushnell's book of "Dark Things." But, however they came, the insects are here, a part of the world of life, growing, multiplying, and dying, like ourselves; undergoing curious transformations, and animated by wonderful instincts—social, industrious, and most instructive in all their ways and history. Dr. Packard selects the most common, those that are easily—often too easily—observed, and gives us their various stories with an interest that is quite romantic. His volume is compact with information upon the subject, and is adapted to all intelligent readers; but, for sensible boys and girls, it is worth a whole library of the fictitious drivel that now forms so large a part of the mental nourishment of the young.

This volume consists mainly of reprinted matter, but it contains a new and admirable chapter entitled "Hints on the Ancestry of Insects." The irrepressible question of origins is not to be escaped, and, as it has long haunted the souls of botanists, it now begins to torment the entomological soul. Insects cannot be studied without being classed, and they cannot be classed without knowing their resemblances and affinities, and these cannot be made out except through their embryological or developmental history. The question how things are runs into the question how they came to be, and the first thesis of Scripture becomes the last problem of science—that is, *genesis*. Dr. Packard inclines to the view that the primal ancestors of insects were worms, and he assumes without hesitation the doctrine of evolution as best explaining the facts of the science. We quote one or two passages upon this point:

> Many short-sighted persons complain that such a theory sets in the background the idea of a personal Creator; but minds no less devout, and perhaps a trifle more thoughtful, see the hand of a Creator not less in the evolution of plants and animals from preëxistent forms, through natural laws, than in the evolution of a summer's shower, through the laws discovered by the meteorologist, who looks back through myriads of ages to the causes that led to the distribution of mountain-chains, ocean-currents, and trade-winds, which combine to produce the necessary conditions resulting in that shower.
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> Indeed, to the student of Nature, the evolution theory in biology, with the nebular hypothesis, and the grand law in physics of the correlation of forces, all independent, and revealing to us the mode in which the Creator of the universe works in the world of matter, together form an immeasurably grander conception of the order of creation and its ordainer than was possible for us to form before these laws were discovered and put to practical use.

Again he says:

> Thus the ovipositor of the bee has a history, and is not apparently a special creation, but a structure gradually developed to subserve the use of a defensive organ. So the organs of special sense in insects are, in most cases, simply altered hairs. The hairs themselves are modified epithelial cells. The eyes of insects, simple and compound, are at first simply epithelial cells, modified for a special purpose; and even the egg is but a modified epithelial cell attached to the walls of the ovary, which in turn is morphologically but a gland. Thus Nature deals in simples, and with her units of structure elaborates as her crowning work a temple in which the mind of man, formed in the image of God, may dwell. Her results are not the less marvelous because we are beginning to dimly trace the process by which they arise. It should not lessen our awe and reverence for Deity if, with minds made to adore, we also essay to trace the movements of His hand in the origin of the forms of life.
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> Some writers of the evolution school are strenuous in the belief that the evolution hypothesis overthrows the idea of archetypes and plans of structure. But a true genealogy of animals and plants represents a natural system, and the types of animals, be they four, as Cuvier taught, or five, or more, are recognized by naturalists through the study of dry, hard, anatomical facts. Accepting, then, the type of articulates as founded in Nature from the similar modes of development and points of structure perceived between the worms and the crustacea on the one hand, and the worms and insects on the other, have we not a strong genetic bond uniting these three great groups into one grand sub-kingdom, and can we not in imagination perceive the successive steps by which the Creator, acting through the laws of evolution, has built up the great articulate division of the animal kingdom?—*The Popular Science Monthly*, 1873–4, **4**, 247.

THE DEPTHS OF THE SEA. By C. WYVILLE THOMPSON, LL.D., etc. London and New York: Macmillan & Co., 1873.

CERTAIN new and very interesting results, in regard to the distribution of life, have been arrived at within the last few years, by dredging the bottom of the sea. Twenty years ago it was believed that at certain depths the greatness of the pressure, the lowness of the temperature, and the deficiency of light and aëration, made it impossible for life to subsist. The alleged cases of living creatures being drawn up from great depths were discredited. The operations of cable-laying and cable-raising have, however, increased our familiarity with the bottom of the sea, and the improved manipulations have been turned to account in exploring its life. The result was the establishment of the truth that there is an order of life belonging to the sea-bed in the profound abysses of the ocean. The recognition of this fact led to systematic attempts to carry on deep-sea explorations. In 1868 the steamer *Lightning* was placed by the British Government at the disposal of Dr. Carpenter and Mr. Wyville Thompson for the express purpose of submarine research, and the *Porcupine* was afterward assigned, for a more extensive series of surveys, to the same gentlemen, with the addition of Mr. Gwyn Jeffreys, in the summers of 1869 and 1870. In the first of these cruises the greatest depth reached was 1,500 fathoms, but in the second they went to the depth of 2,500 or 3,000 fathoms. The present volume is a record of the

results attained in these expeditions. It gives an account of the apparatus and instruments employed, of the forms of organization discovered, and much information regarding the physics of the ocean. It is splendidly illustrated and popularly written, with much humor, and the treatment, like the subject, is anything but dry; it is a volume altogether worthy the interest and importance of its subject.—*The Popular Science Monthly*, 1873, **3**, 122.

HEALTH AND EDUCATION. By the Rev. CHARLES KINGSLEY, F.L.S., F.G.S., Canon of Westminster. New York: D. Appleton & Co., 549 & 551 Broadway. Pages 411. Price, $2.00.

THIS is a unique volume from the vigorous and brilliant pen of the versatile Canon of Westminster, novelist, essayist, naturalist, professor of history, and preacher, and so strong in each as to have won a commanding place in the literature of the time. Mr. Kingsley has here given us the result of his long observations and reflections on the theory, philosophy, and practical conduct of life. His work is popular in the highest sense; that is, it is not only designed for general influence, but it is done in its author's best style of literary art, and is vivid, quaint, pungent, and impressive. It is well known that Canon Kingsley is one of the masters of the English language, and it is fortunate when he brings his unusual powers of presentation to bear upon familiar and important subjects of daily life. For the difficulty with people generally is, not that they are ignorant, or have not had truth enough explained to them, but that it is so vaguely conceived and so feebly held that it does not take hold of the feelings and coerce the conduct. For this reason, much of the tame didactic statement of current science is to a great degree powerless for good. It is here that the forcible, pointed, and picturesque writer is of invaluable service, and it is here that Canon Kingsley excels. The contents of the volume are varied and suggestive, and it abounds in passages of pointed common sense, like the following fresh plea for the practical study of botany by girls, as grounds of important mental discipline:

> Mothers complain to me that girls are apt to be—not intentionally untruthful—but exaggerative, prejudiced, incorrect, in repeating a conversation or describing an event; and that from this fault arise, as is to be expected, misunderstandings, quarrels, rumors, slanders, scandals, and what not.
>
> Now, for this waste of words there is but one cure: and if I be told that it is a natural fault of women; that they cannot take the calm, judicial view of matters which men boast, and often boast most wrongly, that they can take; that under the influence of hope, fear, delicate antipathy, honest moral indignation, they will let their eyes and ears be governed by their feelings; and see and hear only what they wish to see and hear: I answer, that is not for me as a man to start such a theory; but that, if it be true, it is an additional argument for some education which will correct this supposed natural defect. And I say deliberately that there is but one sort of education which will correct it; one which will teach young women to observe facts accurately, judge them calmly, and describe them carefully, without adding or distorting: and that is, some training in natural science.
>
> I beg you not to be startled: but if you are, then test the truth of my theory by playing to-night at the game called "Russian Scandal;" in which a story, repeated in secret by one player to the other, comes out at the end of the game, owing to the inaccurate and—forgive me if I say it—uneducated brains through which it has passed, utterly unlike its original; not only ludicrously maimed and distorted, but often with the most fantastic additions of events, details, names, dates, places, which each player will aver that he received from the player before him. I am afraid that too much of the average gossip of every city, town, and village is little more than a game of "Russian Scandal;" with this difference, that, while one is but a game, the other is but too mischievous earnest.
>
> But now, if among your party there should be an average lawyer, medical man, or man of science, you will find that he, and perhaps he alone, will be able to retail accurately the story which had been told him. And why? Simply because his mind has been trained to deal with facts; to ascertain exactly what he does see or hear, and to imprint its leading features strongly and clearly on his memory.
>
> Now, you certainly cannot make young ladies barristers or attorneys; nor employ their brains in getting up cases, civil or criminal; and as for chemistry, they and their parents may have a reasonable antipathy to smells, blackened fingers, and occasional explosions and poisonings. But you may make them something of botanists, zoologists, geologists.
>
> I could say much on this point: allow me at least to say this: I verily believe that any young lady who would employ some of her leisure time in collecting wild-flowers, carefully examining them, verifying them, and arranging them; or who would in her summer trip to the sea-coast do the same by the common objects of the shore, instead of wasting her holiday, as one sees hundreds doing, in lounging on benches on the esplanade, reading worthless novels, and criticising dresses—that such a young lady, I say, would not only open her own mind to a world of wonder, beauty, and wisdom, which, if it did not make her a more reverent and pious soul, she cannot be the woman which I take for granted she is; but would save herself from the habit—I had almost said the necessity—of gossip: because she would have things to think of and not merely persons; facts instead of fancies; while she would acquire something of accuracy, of patience, of methodical observation and judgment, which would stand her in good stead in the events of daily life, and increase her power of bridling her tongue and her imagination. "God is in heaven, and thou upon earth; therefore let thy words be few;" is the lesson which those are learning all day long who study the works of God with reverent accuracy, lest by misrepresenting them they should be tempted to say that God has done that which he has not; and in that wholesome discipline I long that women as well as men should share.

In his lecture on the Tree of Knowledge, Mr. Kingsley has the following observations on the causes of intemperance:

> It is said by some that drunkenness is on the increase in this island. I have no trusty proof of it: but I can believe it possible; for every cause of drunkenness seems on the increase. Overwork of body and mind; circumstances which depress health; temptation to drink, and

drink again, at every corner of the streets; and finally, money, and ever more money, in the hands of uneducated people, who have not the desire, and too often not the means, of spending it in any save the lowest pleasures. These, it seems to me, are the true causes of drunkenness, increasing or not. And if we wish to become a more temperate nation, we must lessen them, if we cannot eradicate them.

First, overwork. We all live too fast, and work too hard. "All things are full of labor, man cannot utter it." In the heavy struggle for existence which goes on all around us, each man is tasked more and more—if he be really worth buying and using—to the utmost of his powers all day long. The weak have to compete on equal terms with the strong; and crave, in consequence, for artificial strength. How we shall stop that I know not, while every man is "making haste to be rich, and piercing himself through with many sorrows, and falling into foolish and hurtful lusts, which drown men in destruction and perdition."

But it seems to me also, that in such a state of society, when—as it was once well put—"every one has stopped running about like rats:"—that those who work hard, whether with muscle or with brain, would not be surrounded, as now, with every circumstance which tempts toward drink; by every circumstance which depresses the vital energies, and leaves them an easy prey to pestilence itself; by bad light, bad air, bad food, bad water, bad smells, bad occupations, which weaken the muscles, cramp the chest, disorder the digestion. Let any rational man, fresh from the country—in which I presume God, having made it, meant all men, more or less, to live—go through the back streets of any city, or through whole districts of the "black countries" of England; and then ask himself—Is it the will of God that his human children should live and toil in such dens, such deserts, such dark places of the earth? Let him ask himself—Can they live and toil there without contracting a probably diseased habit of body; without contracting a certainly dull, weary, sordid habit of mind, which craves for any pleasure, however brutal, to escape from its own stupidity and emptiness? When I run through, by rail, certain parts of the iron-producing country—streets of furnaces, collieries, slag-heaps, mud, slop, brick house-rows, smoke, dirt—and that is all; and when I am told, whether truly or falsely, that the main thing which the well-paid and well-fed men of those abominable wastes care for is—good fighting dogs: I can only answer, that I am not surprised.

I say—as I have said elsewhere, and shall do my best to say again—that the craving for drink and narcotics, especially that engendered in our great cities, is not a disease, but a symptom of disease; of a far deeper disease than any which drunkenness can produce; namely, of the growing degeneracy of a population striving in vain by stimulants and narcotics to fight against those slow poisons with which our greedy barbarism, miscalled civilization, has surrounded them from the cradle to the grave. I may be answered that the old German, Angle, Dane, drank heavily. I know it: but why did they drink, save that for the same reason that the fenman drank, and his wife took opium, at least till the fens were drained? why but to keep off the depressing effects of the malaria of swamps and new clearings, which told on them—who always settled on the lowest grounds—in the shape of fever and ague? Here it may be answered again, that stimulants have been, during the memory of man, the destruction of the Red Indian race in America. I reply boldly, that I do not believe it. There is evidence enough in Jacques Cartier's "Voyages to the Rivers of Canada;" and evidence more than enough in Strachey's "Travaile in Virginia"—to quote only two authorities out of many—to prove that the Red Indians, when the white man first met with them, were, in North and South alike, a diseased, decaying, and, as all their traditions confess, decreasing race. Such a race would naturally crave for "the water of life," the "usquebaugh," or whiskey, as we have contracted the old name now. But I should have thought that the white man, by introducing among these poor creatures iron, fire-arms, blankets, and above all, horses wherewith to follow the buffalo-herds which they could never follow on foot, must have done ten times more toward keeping them alive, than he has done toward destroying them by giving them the chance of a week's drunkenness twice a year, when they came in to his forts to sell the skins which, without his gifts, they would never have got.

Such a race would, of course, if wanting vitality, crave for stimulants. But if the stimulants, and not the original want of vitality, combined with morals utterly detestable, and worthy only of the gallows—and here I know what I say, and dare not tell what I know, from eye-witnesses—have been the cause of the Red Indians' extinction: then how is it, let me ask, that the Irishman and the Scotsman have, often to their great harm, been drinking as much whiskey—and usually very bad whiskey—not merely twice a year, but as often as they could get it, during the whole "iron age;" and, for aught any one can tell, during the "bronze age," and the "stone age" before that: and yet are still the most healthy, able, valiant, and prolific races in Europe? Had they drunk less whiskey they would, doubtless, have been more healthy, able, valiant, and perhaps even more prolific, than they are now. They show no sign, however, as yet, of going the way of the Red Indian.

But if the craving for stimulants and narcotics is a token of deficient vitality; then the deadliest foe of that craving, and all its miserable results, is surely the Sanitary Reformer; the man who preaches, and—as far as ignorance and vested interests will allow him—procures, for the masses, pure air, pure sunlight, pure water, pure dwelling-houses, pure food. Not merely every fresh drinking-fountain: but every fresh public bath and wash-house, every fresh open space, every fresh growing tree, every fresh open window, every fresh flower in that window—each of these is so much, as the old Persians would have said, conquered for Ormuzd, the god of light and life, out of the dominion of Ahriman, the king of darkness and of death; so much taken from the causes of drunkenness and disease, and added to the causes of sobriety and health.

Meanwhile one thing is clear: that if this present barbarism and anarchy of covetousness, miscalled modern civilization, were tamed and drilled into something more like the kingdom of God on earth: then we should not see the reckless and needless multiplication of liquor-shops which disgraces this country now. . . .

I said just now that a probable cause of increasing drunkenness was the increasing material prosperity of thousands who knew no recreation beyond low animal pleasure. If I am right—and I believe that I am right—I must urge on those who wish drunkenness to decrease, the necessity of providing more, and more refined, recreation for the people.

Men drink, and women too, remember, not merely to supply exhaustion; not merely to drive away care: but often simply to drive away dullness. They have nothing to do save to think over what they have done in the day

or what they expect to do tomorrow; and they escape from that dreary round of business thought, in liquor or narcotics. There are still those, by no means of the hand-working class, but absorbed all day by business, who drink heavily at night in their own comfortable homes, simply to recreate their overburdened minds. Such cases, doubtless, are far less common than they were fifty years ago: but why? Is not the decrease of drinking among the richer classes certainly due to the increased refinement and variety of their tastes and occupations? In cultivating the aesthetic side of man's nature; in engaging him with the beautiful, the pure, the wonderful, the truly natural; with painting, poetry, music, horticulture, physical science—in all this lies recreation, in the true and literal sense of that word, namely, the recreating and mending of the exhausted mind and feelings, such as no rational man will now neglect, either for himself, his children, or his workpeople.

—*The Popular Science Monthly*, 1874, **5**, 632.

# POPULAR MISCELLANY

**Reopening of an Old Route into Siberia.**—Fully three hundred years ago the Russians carried on an extensive trade between Archangel and the settlements on the Obi and Yenisei. About the same period the Kara Sea was navigated by English and Dutch mariners, in search of a northeast passage to Japan. The Russians employed wretched flat-bottomed boats, called *kotchkies*, and in these they braved all the dangers of navigating the stormy Kara Sea. But, till quite lately, this route to the interior of Siberia was abandoned, and the belief was generally entertained that the existence of ice in the Kara Sea presented an insuperable obstacle to navigation. Recent expeditions to the mouths of the Obi and Yenisei, and up those rivers for hundreds of miles, have demonstrated the entire feasibility of this route to the interior of Siberia. The influence of the Gulf Stream and equatorial currents on the temperature of the Kara Sea is apparent from the fact that its waters are as much as 18° or 20° warmer than the waters in the same latitudes off the east coast of Greenland or in Davis's Strait. Of Siberia, the country to be opened up to commerce by the navigation of the Kara Sea, M. de Lesseps declares that it is the richest country in the whole world as regards its vegetable, mineral, and animal products. The great rivers of Siberia flow from the south to the north, forming a vast fan which widens in the interior of the country, to the great advantage both of vegetation and of commerce. The Obi, with its confluent the Irtish, affords a navigable highway into China.

**A Plague of Rabbits in New Zealand.**—Some years ago rabbits were introduced into South Australia from England, later, a like importation was made into New Zealand. Now these rodents are a formidable pest in those countries, and it has become a question of extreme urgency how they can be exterminated. In New Zealand a commission has been instituted by the government to inquire into the subject, and devise a remedy. Already, though only a few years have passed since the introduction of the rabbits, large tracts of rich pasture land have been converted into wilderness, and sheep-farming and cattle-raising are becoming impossible. Farmers that used to keep 15,000 or 16,000 sheep can now hardly keep as many hundred. Landowners employ men and dogs to destroy the rabbits, but, though the number killed is enormous, the evil continues without serious abatement. One landowner inclosed with a stone wall an area of 10,000 acres, the work taking seven years to complete, and involving an expenditure of £35,000. About 500,000 rabbitskins were exported from Hobart Town in 1874. It is proposed to introduce from England, if possible, several natural enemies of the rabbit, such as stoats, weasels, ferrets, and hawks.

**Latest Phase of the Spontaneous-Generation Controversy.**—Dr. Bastian, of London, having submitted to the Paris Academy of Sciences the results of certain experiments which, as he maintains, decisively confirm his theory of spontaneous generation, Pasteur criticised the English investigator's methods and conclusions, and asked for the appointment of a commission to determine on which side the truth lies. At the same time he expressed a wish that Dr. Bastian should in like manner ask the London Royal Society to appoint a similar commission. According to the terms of M. Pasteur's challenge, Dr. Bastian must obtain, in the presence of competent judges, bacteria in sterile urine on the addition of liquor potassæ in suitable quantities, the liquor potassæ being prepared from pure potash, with pure water, or if made from impure materials, it must be submitted to a temperature of 230° for twenty minutes. Dr. Bastian has accepted the challenge, and has applied to the Royal Society for the appointment of the commission. The French commission is already constituted: it consists of Milne-Edwards, Dumas, and Boussingault. The *Lancet* justly complains against this selection, on the ground that all of the three commissioners are more or less strong supporters of Pasteur's view. Their bias must inevitably indispose them toward Bastian's arguments. The *Lancet* asks why Frémy or Trécul, or some other man without bias either way, was not placed on the commission. The Academy has apparently made a mistake in this matter; perhaps when the comments of the *Lancet* are brought to the notice of the members, a change will be made in the commission. The Royal Society has not yet named the members of the English commission.—*The Popular Science Monthly*, 1877, **11**, 378, 379, 381.

# CORRESPONDENCE

## THE DANGERS AND SECURITIES OF SCIENCE

*To the Editor of the Popular Science Monthly.*

MR. EDITOR: I listened, among others, to the speech of Mr. Parke Godwin at the Tyndall Dinner, and have been much interested both in the speech and in the discussions which have grown out of it. Of course, we cannot all expect to view the most important subjects in the same light, but I feel sure it is a mistake to attribute to Mr. Godwin anything like a spirit of opposition or depreciation toward scientific progress or preëminence. On the contrary, what he said was, we believe, wholly in the interest of science. He simply gave expression, in unusually elegant and forcible language, to ideas which are entertained of late by many professionally scientific men. He did not propose to cramp scientific inquiry, nor to limit, in any way, its powers or its results, but only to prevent its contamination by what would degrade and cripple it. His speech, as we understand it, was a protest, not against science, but in its behalf, and against the damaging influence of pretended followers or mistaken friends.

There is no danger now that science can ever suffer from the attacks of its enemies, unless it be first debauched by the folly of its own partisans. Its progress for the last hundred years has been a series of triumphs, so numerous and brilliant that nothing else is now in a position to stand against it. And it owes this success entirely to the fidelity with which it has pursued its legitimate course, and the steady determination with which it has adhered to the method of strict scientific observation. For a long time we have given up the notion of the old philosophers, that men could discover things by thinking about them; and have only considered it worth while to spend our time in the investigation of actual phenomena. What has been, for the last half-century, the invariable demand of the world of science upon its votaries? Whenever anyone made his appearance with a new claim to attention, the scientific public said to him, in effect: "What is that you have to tell us of this new body or substance? We do not wish to hear what you *think* about it, but only what you *know*. How much does it weigh? What are its form and structure? What are the actual results of its chemical analysis? What phenomena does it exhibit under special conditions? If it be a peculiar force or mode of activity, instead of a material substance, what are the exact conditions of its manifestation, and what are the results of its action, in quantity as well as in kind?"

This is the healthy and nutritious food upon which science has grown to her present proportions. In following such a track with such unswerving patience, she can never make a mistake. But the moment she leaves this path, she is in danger, or rather she is sure to go wrong, because whatever works by other than scientific methods is not science, and at best can only put on a kind of scientific garb, and masquerade in scientific phraseology.

Are there not some indications that we are not yet altogether beyond this danger? Are we not even more or less exposed to it at this particular time? Some scientific writers are certainly disposed to talk quite as much about their conclusions and theoretical explanations as about the phenomena they describe. There is no harm in this (except that it occupies a good deal of time that might be otherwise employed), provided they keep the boundary line well marked between what they know and what they think on the subject in question. But they do not always do this. The hypothetical explanations are sometimes erected into a law, or principle, or theory, which, in the author's mind, evidently overshadows in importance everything else. So we are sometimes supposed to have acquired a valuable piece of information when we are only, as the French say, "getting our pay in words." How much has been said and written for the past few years about *protoplasm!* Now, a student of physiology would be very excusable for thinking, from the manner in which this term is used, that protoplasm was some newly discovered and important substance, with definite physical and chemical properties, and of the highest consequence in regard to vital organization. He would be considerably disappointed on finding it to be only a word representing a certain set of ideas, or at best a group of many various substances, each one of them specifically different from the rest.

There is even a certain kind of authority claimed, at least by implication, for some of these theoretical notions; and there is no doubt that they are occasionally assigned an established position as accepted truths, to which they are very far from being entitled. If it were not so improbable that science could ever be induced to imitate in the least degree her old theological enemy, we might suspect even now a disposition in some minds to frame for us a sort of scientific Nicene Creed, the merit of believing in which would not depend exclusively upon the possession of sufficient reliable evidence. If such a creed were drawn up just at present, it would probably read something like this:

I BELIEVE in the Darwinian Theory;
In the Evolution Hypothesis;
In the Undulation of Light and the Luminiferous Ether; and
In the Atomic Constitution of Matter.

Now, we all know that theories are useful in their way, if confined within a very small compass, and employed only to stimulate rather than satisfy inquiry, and to suggest the direction in which new facts may be discovered. But, when they are raised to a higher dignity, and demand our belief in them as representing the actual constitution of Nature, then they are a misfortune

to everybody concerned. If we treat them with any more respect than they deserve, we shall suffer for it inevitably by the loss of something which is infinitely more valuable than any of them. The records of the immediate past show the achievements which have been accomplished by means of strict adherence to exact methods of investigation. Should the scientific mind of today become ever so little intoxicated with its success, and undertake to decide questions which are beyond its horizon, it will certainly stultify itself, and lose the universal support and confidence which it has now so fairly acquired. For that reason I think that Mr. Godwin, in his Tyndall Dinner speech, was doing good service for science and scientific men, and that we are indebted to him for placing in a very distinct light the only source of danger for scientific interests in the future.—J.C.D.

*The Popular Science Monthly,* 1873, **3**, 240.

## EVOLUTION IN AMHERST COLLEGE

*Messrs. Editors.*

I HAVE the best authority for saying that President Seelye, of Amherst College, is not satisfied with the interpretations which have been put upon his letter to the "Observer," to the effect that "groundless guesses" are not yet taught at this college "as ascertained truths of science," and that the doctrine of the evolution of man, from the monkey or from any of the irrational animals, being in flat contradiction to all the facts of history, is only fit to be left with the sciolists. He is not willing that a wrong impression should be given to the public with respect to his attitude toward science in general and the scientific doctrines of evolution in particular; and he claims that it was very far from his intention, in his note to the "Observer," to place his college or himself in antagonism to either. It is only just to him, therefore, that he should be set right. I am authorized to say that he has long "firmly believed the current doctrine of evolution to contain a great truth, as well as a subtile error." Moreover, he maintains that "all great truths, whether styled evolution or otherwise, so far as they are truly scientific, are taught and encouraged at Amherst; that all investigations of science and all earnest efforts to widen the field of our knowledge find a cordial welcome there. In a word, that evolution, cosmological and biological, so far as it is scientific, is taught as a part of science; but that such atheistic, illegitimate, unscientific conclusions or assumptions as may apparently flow from certain unwarranted expositions of the law of evolution, together with all other illogical, irrational, and unscientific conclusions and dogmatic deliverances constitute the 'groundless guesses' and the 'subtile error,' " to which he referred —"and that these find no favor at Amherst." President Seelye gives his unqualified assent to and adopts this statement of his own, and the position of the college, and thinks its publication will do good.

Besides, it appears that Le Conte's "Geology" was specially recommended by the President to the Professor of Geology as a textbook for his class before its introduction, and after a careful examination of the work. Moreover, that the new department of biology was introduced to the college through President Seelye's own proposal and urgency with the trustees, and this because he desired the college to have the best and largest teaching in so important a field; also that he turned the attention of the present instructor in biology in that particular line, and sent word to him while in Germany that he should especially make the acquaintance of Haeckel and his work. The President considers that he would be unworthy of his place if he were indifferent to or intolerant of the investigations of science, and expresses himself as the reverse of hostile to free thinking.

No lover of science can find any fault with the position of President Seelye in its new aspect, and it is certainly a matter for regret that he has been placed at all in a false or equivocal attitude. Of course, no one who knows him can for an instant entertain the thought that he intended to mislead or supposed he would mislead by his former statement.

Upon such a platform, if faithfully adhered to and its declarations rigorously carried out, and under such leadership, Amherst will certainly be entitled to a high place among those institutions of broad and thorough culture and catholicity of sentiment of which Harvard and the Johns Hopkins University are prominent examples—institutions which inspire confidence in American scholarship at home and give it character abroad. And the evolutionists at Amherst, who are working in their special departments, are to be most heartily congratulated that, unlike their less fortunate brethren at Yale, *they* at least have no one more ready to support them and lead them forward than the President of their own college, who says it is his "full belief that there was never more or better scientific instruction given in Amherst than now."—DANIEL G. THOMPSON.

New York City, *May 20,* 1880

*The Popular Science Monthly,* 1880, **17**, 405.

## THE KELLEY'S ISLAND GROOVE

*Editor Popular Science Monthly:*

DEAR SIR: In this month's number of the Science Monthly, under the "Miscellaneous" head, you have a notice of the work now in progress for the preservation of the great glacial groove on Kelley's island.

In that notice you speak of Prof. Wright and Dr. Sprecher as having "surveyed" the plot of land on which the groove is located. In this statement you are in error. They are *not* surveyors, and they did not survey the plot, and the suggestion of such an occupation for them must seem to those who know them very inappropriate. Prof. Wright is Professor of "New Testament Greek" at Oberlin, and the author of that noble book, The Ice Age in North America, published by the Appletons in 1890; and Dr. Sprecher is pastor of one of the largest Presbyterian churches in our city. And in that notice you make another error, which to me seems very absurd. You give my name as Young*blood.* It is not Young*blood,* as you may learn from your subscription



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list, where it has been recorded from the time that the first number of the Science Monthly was issued.

The facts are just these; my invitation to Prof. Wright and Dr. Sprecher to visit the island with me was wholly a matter of courtesy. While there I consulted them as to the best method of protecting the groove from the incursions of the Vandal curiosity-hunters, and also as to the best form of conveying the title, to be held in perpetuity for the benefit of science; and all of the surveying that was done by those gentlemen they did with their eyes, as they stood admiring that beautiful and wonderful work of Nature's laws.

I take pleasure in saying that I have completed the work of uncovering fifty feet of the groove, leaving fifty feet still covered to the depth of about twelve feet with clay, gravel, and fragments of the lime rock, just as it was left by Nature's laws when their work was finished, and the tools with which that work was done—granite bowlders—lie scattered over the island, and on the mainland, as far west as the Indiana line, there to rest, imperishable and unchanged, until Nature shall again take them up to do its work.

Were you to see that groove at this time I feel sure that you would pronounce it to be the most beautiful and wonderful evidence of the glacial movement that has ever been brought to the notice of civilized man.

On the 237th page of Prof. Wright's Ice Age there is an engraving which gives an imperfect view of the easterly end of the great groove, as it appeared before it was uncovered. And on the 238th page of the same book there is an engraving of another grooved rock, which is a little north of the great groove, from which I had taken off about a hundred feet before the photograph was taken, and sent to various scientific institutions. This, too, . . . is a beautiful specimen of Nature's work.

I beg that you will pardon me for troubling you with this letter, for I feel that it is due to my friends and also to myself that the errors which I have noted should be corrected.

And, now that I have nothing further to say on the subject which prompted this letter, I will add a few words regarding The Popular Science Monthly. I have been a subscriber from the time of the issue of the first number, and I now have thirty volumes bound; and I take pleasure in saying that I think that there are no other thirty volumes to be found which contain such a vast and varied amount of useful information, or which are so well calculated to educate men in matters which advance our civilization, as those.

And more—they are a most noble monument to "Edward L. Youmans," more beautiful and enduring than marble or granite.

I am, sir, very respectfully yours,

M. C. YOUNGLOVE

Cleveland, *September* 16, 1891

[The paragraph noticed by Mr. Younglove was compiled from a slip which was sent to the Monthly from a Cleveland paper. The language of the slip was followed, without supposing that the word "surveyed" was meant to be used in a technical sense, but rather perhaps in its original sense of looked-over, or perhaps as meaning that Drs. Wright and Sprecher had the ground surveyed. The change of our correspondent's name to Youngblood was one that we much regret; but it was also one that might naturally occur in transcription or type-setting and be overlooked by a stranger to the person concerned; for to a stranger no suggestion of error would be likely to occur.]

*The Popular Science Monthly,* 1892, **40**, 267.

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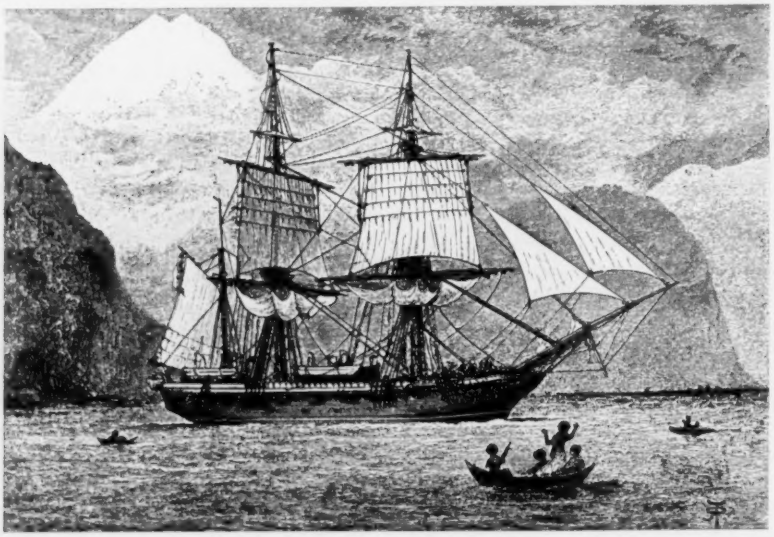

H. M. S. *Beagle* in Straits of Magellan—Mount Sarmiento in the distance.
Reproduced from *The Popular Science Monthly,* 1890, **36**, 750.